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DOCILITY AND PURPOSIVENESS

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In two earlier papers,[1] I have discussed various conceptions which have been proposed as definitions of purpose, such as *systematic unity*, *tendency*, and *adaptation*. None of these appears to give any meaning to such phrases as 'in order to' or 'for the sake of,' which I have selected as the most unmistakably and unqualifiedly teleological expressions in common use. Adaptation, or complementary adjustment may, if one so desires, be regarded as purposive in a broad sense. But I have thought it important to show that such processes may be construed as complex cases of ordinary automatism or mechanism. Complementary adjustment means a give and take between environment and organism, in which the environment makes the first move, and in which the exchange of actions obeys a constant law. Given the law, and any state of the varying environment (stimulus), the response of the organism can be predicted. The law itself is like any mechanical law, and is simply obeyed by the responding organism. The response *is* complementary; but it means nothing to say that it occurs *in order to be* complementary, or *for the sake of* the complementary outcome.

In the case of plastic or modifiable behavior, we meet with a new and important principle. The organism *acquires* or *learns* complementary adjustments. The proverbial burnt child, for example, acquires a response that is appropriate to the stimulus of fire. It is not that the child does so

[1] Purpose as Systematic Unity, *Monist*, 1917, 27, 352–375; and Purpose as Tendency and Adaptation, *Phil. Rev.*, 1917, 26, 477–495.

respond, but that the response has been selected *owing to* its complementary character. To do full justice to the complexity of this total process it is necessary to recognize two propensities, which we may for the present call the *selective* or *higher* propensity and the *eligible*, or *lower* propensity. Let us illustrate these from a case of animal learning described by Professor Thorndike.

We take a box twenty by fifteen by twelve inches, replace its cover and front side by bars an inch apart, and make in this front side a door arranged so as to fall open when a wooden button inside is turned from a vertical to a horizontal position. . . . A kitten, three to six months old, if put in this box when hungry, a bit of fish being left outside, reacts as follows: It tries to squeeze through between the bars, claws at the bars and at loose things in and out of the box, stretches its paws out between the bars, and bites at its confining walls. Some one of all these promiscuous clawings, squeezings and bitings turns round the wooden button, and kitten gains freedom and food. By repeating the experience again and again, the animal comes gradually to omit all the useless clawing and the like, and to manifest only the particular impulse (*e. g.*, to claw hard at the top of the button with the paw, or to push against one side of it with the nose) which has resulted successfully. It turns the button around without delay whenever put in the box.[1]

The eligible propensity in this case is the acquired propensity or habit which proves 'successful,' such as clawing hard at the top of the button with the paw. This eligible propensity is complementary to the environment in that it so combines with the environment and with reflexes such as seizing, chewing and swallowing, as to restore the vitality of the hungary organism to par. But in the course of acquiring this propensity the kitten cannot as yet be determined by it. We need therefore to recognize another or higher propensity to account for the kitten's 'trying.' This higher or selective propensity dominates the whole process. It accounts for the animal's activity, and it also accounts for that form of activity which is chosen to be the stereotyped and recurrent activity. It excites the animal to efforts that continue *until* a certain specific act occurs; and it determines what character that specific act shall possess in order to become recurrent. In the maxim 'if at first you don't succeed, try, try again,' the higher propensity both accounts for the re-

[1] E. L. Thorndike, 'Educational Psychology,' Vol. II., The Psychology of Learning, p. 9.

peated trials and defines what shall constitute success. Or, in the saying 'he won't be happy till he gets it,' the selective propensity accounts for the unhappiness, and for that specific thing which alone will remove it.

Although its peculiar importance has not, I think, been recognized, this governing or selective propensity is familiar enough to psychologists. Professor Thorndike calls it "the learner's *Set* or *Attitude* or *Adjustment* or *Determination*."[1] Professor Woodworth describes it as follows:

> We must assume in the animal an adjustment or determination of the psychophysical mechanism toward a certain end. . . . His behavior shows that he is, as an organism, set in that direction. This adjustment persists till the motor reaction is consummated; it is the driving force in the unremitting efforts of the animal to attain the desired end.[2]

It is manifestly the same thing which some psychologists refer to as conation. Speaking of instinctive behavior, Professor McDougall says:

> The process, unlike any merely mechanical process, is not to be arrested by any sufficient mechanical obstacle, but is rather intensified by any such obstacle and only comes to an end either when its appropriate goal is achieved, or when some stronger incompatible tendency is excited, or when the creature is exhausted by its persistent efforts.[3]

Professor Watson has recently attempted a rigorously physiological interpretation of the learning process. He is especially anxious to avoid any appeal to conscious guidance, or to conscious pleasure and displeasure, and 'to account for the elimination of useless movements upon purely objective grounds.'[4] It is not clear that he would wish to reject teleology of the sort that is here proposed. In any case, I cannot see that his account of the matter, assuming it to be correct, is in essential disagreement with the above analysis.

Professor Watson proposes to account for the formation of habit solely by the laws of *frequency* and *recency*. The

[1] *Op. cit.*, p. 13.

[2] G. T. Ladd and R. S. Woodworth, 'Elements of Physiological Psychology,' p. 551.

[3] W. McDougall, 'Social Psychology,' p. 27. Professor McDougall, to be sure, reserves the term 'conation' for the consciousness or 'experience' which he believes attends behavior of this type; but in any case the process described above would represent the behaviorist aspect or criterion of conation.

[4] 'Behaviorism,' p. 251.

so-called 'successful' movement of the animal is the movement which terminates each trial, as does movement 10 in the series 1, 2, 3, 4, 5, 6, 7, 8, 9, *10* and 1, 3, 7, 13, 14, 8, *10*. Where there are repeated trials, *10* is then the movement which occurs in all, and hence most frequently; and it is the movement which, upon each new trial, has occurred most recently. Hence it comes to occur more promptly with each successive trial, and eventually to become a fixed habit.

But the important point, as I see it, is the fact that movement *10 does terminate* each trial, without the interference of the experimenter. A series of movements is repeatedly inaugurated, and each series continues until a certain end-state is reached. The animal then ceases to try. The important phenomenon is that of effort persisting until a situation is reached which evokes no further response; or which inaugurates a new series of trials which terminate differently, and lead to the formation of another and independent habit. The learning animal is in each case being *driven* by something (as when 'hunger is driving the animal' [1]), the peculiar reactivity of the animal being due not merely to a specific stimulus-reflex, but to a general propensity which has its own specific and determined lapse or quietus.

Professor Holt, in a recent discussion of 'The Physiology of Wishes,' has followed Sherrington and Freud in emphasizing the antagonism, reinforcement and integration of reflexes. He rightly insists that the behavior of a living organism is a *doing of something*, and is therefore describable only by reference to that environmental object toward which the act addresses itself. Even simple reflexes have this character of transcending the organism in which they occur. All responses are responses 'to' something, and that something is a part of their essential character. Where two or more responses are excited they may augment one another, inhibit one another, or give way to a resultant response in which they are both partially present. The process of integration is fairly intelligible in terms of muscular antagonism, resistance in the nerve cells or synapses, and distribution of nervous energy.

[1] According to Professor Watson himself. Cf. *ibid.*, p. 265, note.

While the view which I am proposing is in fundamental agreement with that of Professor Holt, there are two important differences of emphasis. In the first place, Professor Holt uses the term 'purpose' for any organic response having specific objective reference. "This thing," he says, "in its essential definition, is *a course of action which the living body executes or is prepared to execute with regard to some object or some fact of its environment*."[1] "The purpose about to be carried out is already embodied in what we call the 'motor attitude' of the neuro-muscular apparatus."[2] The author accepts Sherrington's statement that "in the light of the Darwinian theory every reflex *must* be purposive."[3] The integrated response is more distinctively purposive only because the objective reference is more unmistakable or more necessary for the definition of the response. "As the number of component reflexes involved in response increases, the immediate stimulus itself recedes further and further from view as the significant factor."[4]

But in the view which I have proposed above the emphasis is placed on the dominance of the general motor set over the subordinate reflexes which are assimilated to it. Professor Holt illustrates his view by the case of the burnt child who learns to respond both to the body's luminousness and also to its hotness. The integrated response is a balance or resultant of the extension reflex and the retraction reflex, 'such that the organism carries on its further examination of the candle in safety.'[5] But if I were using this illustration I should call attention to the dominance of the exploration or curiosity reflex, and say that the child learns how to satisfy that impulse. To extend the hand to the vicinity of the object, to look at it more attentively, or in the case of a more advanced intelligence, to put on asbestos gloves—these are means by which the organism learns to complete or facilitate its primary response. The organism is acting under

[1] 'The Freudian Wish,' ch. 2 on The Physiology of Wishes, pp. 56–57.

[2] *Op. cit.*, p. 59.

[3] *Op. cit.*, p. 66.

[4] *Op. cit.*, pp. 76–77.

[5] *Op. cit.*, p. 73.

the control of one response; and this response is modified and amplified by the absorption or rejection of other responses which are incidental to the general reactivity which the primary response incites. In this way it is possible to attach some significance to the terms 'means' and 'end' which tend to drop out altogether in Professor Holt's account.[1] But the case of the burnt child would be doubtfully purposive in this sense, and I should not regard it as a peculiarly illuminating example of the principle. The type of learning process with which I propose to identify purpose, is better exemplified by cases like the one cited from Thorndike, in which there is clear evidence of a strong and persistent impulse, like hunger; which gradually articulates and completes itself, by communicating its energy to reflexes which facilitate it, and turning its energy against reflexes which retard it. In such cases an organism not only does something, but *it learns how to do something;* the 'how' being selected and consolidated under the control of the 'something-to-be-done.'

Or, still more unmistakable examples abound in the operations of more advanced intelligences. If one is hunting for a pin, the 'Aufgabe' is clearly in command of the situation from beginning to end. Sundry responses, such as walking about, probing corners, lifting objects, etc., are *sub*ordinate, and not *co*ördinate reflexes. They are due to the increased reactivity to which the problem gives rise; they acquire a liability to recurrence according as they do or do not facilitate the finding; interruptions are repelled; and eventually there is built up the integrated response of finding a pin. There

[1] Cf. *op. cit.*, pp. 100–101: "The only semblance of 'end' is found where one purpose is yoked into the service of another purpose, and here the latter might roughly be called the 'end' of the former; yet only roughly and inexactly so, since the whole is process and the subordinate purposes are only its articulate phases." But there must be some difference between the superior purpose and the subordinate purposes, for two reasons: first, because the former exists before the latter, and determines their selection; second, because the subordinate purposes may afterwards be replaced, in which case the superior purpose is not changed but rather is more effectually 'realized.' I think it more in keeping with usage, and on the whole more illuminating, to reserve the term 'purpose' for the superior response, in respect of the selective and controlling function which it exercises.

would be a specific difference between hunting a pin for the sake of exercise, and moving about for the sake of finding a pin; and this difference would depend upon the origin of the general excitation, the tendency to fixation in the minor reflexes, and the parts of the whole process which would be interchangeable.

The second respect in which I should depart from Professor Holt's statement concerns the object of response. Professor Holt speaks of the organism's responding to 'some object or fact of its environment.' That the response is often directed to a non-existent or generalized object, would not, I presume, be denied by Professor Holt. But this is a matter of great importance for the theory of value, and needs greater emphasis. In the case of hunting for a pin, the organism is not, strictly speaking, responding to an object or fact of its environment. The organism is not hunting for any particular pin; and is quite capable of carrying on the hunt, even though there be as a matter of fact no pin in its environment. The finding of any particular pin is the *hypothetical* complement to its present response. It is related to it as a hypothetical key is related to some lock which it *would* fit if it did exist. We cannot deal adequately with this matter here, but it evidently requires an epistemological construction that lies beyond the scope of a strictly physiological behaviorism. The recognition of this fact, though it does not, I think, in the least contradict the fundamental thesis of behaviorism, does forbid any hasty or contemptuous dismissal of the traditional association of purpose with non-physical or 'ideal' entities. And it suggests the danger of confining our analysis too closely to the lower forms of mind. As a matter of fact most human purposes deal with 'objects' of hope, fear, or aspiration that find no place at all in the field of nature as that is defined by the physical sciences.

Let us now consider some recent accounts of the learning process offered by a group of experimentalists in animal psychology.[1] "Apparently we have to do," says Professor S. J. Holmes, "with a selective agency which preserves or

[1] L. T. Hobhouse, S. J. Holmes, J. Peterson, and others.

repeats certain activities and rejects others on the basis of their results."[1] All the writers of this group avoid the popular explanation in terms of the pleasurableness and painfulness of the results, and for a cumulative variety of reasons. Such an explanation imputes causal efficacy to mental states; and it introduces into the field of animal psychology a mental factor which, because it is incapable of objective description, is inconsistent with the accepted technique of the science. Furthermore, to explain behavior by pleasure and pain is to commit the fallacy of *obscurum per obscurius.* Feeling still remains the *terra minime cognita* of psychology. The behaviorist hopes to be able to throw light on the physiological correlates of pleasure and pain, rather than to receive light from the very inconclusive theories on that subject which have already been proposed.[2]

Instead, however, of falling back upon the simpler mechanical explanation in terms of frequency, recency and intensity,[3] the writers of the present group emphasize the mutually reinforcing and inhibiting relations of responses. The earliest statement of this view is to be found in Hobhouse's 'Mind in Evolution.'[4] When the chick pecks at the yolk of egg or at green caterpillars, 'the "result"—the tasting or swallowing—is such as to *confirm* the original mode of reaction'; whereas when the chick pecks at orange peel or cinnabar larvæ, the effect is to *inhibit* the original reaction. For the future the chick is more likely to peck at objects of the first sort, because the excitement which they would arouse independently is now enhanced by the 'assimilation' of the excitement characteristic of the confirmatory sequel. Similarly,

[1] Pleasure, and Pain and Intelligence, *Comp. J. of Neurol.*, 1910, **20**, p. 147.

[2] Cf. J. Peterson, Completeness of Response, Psychol. Rev., 1916, **23**, 157–158: "The pleasurable tone which accompanies certain of our acts is of course only a subjective indication that the response is along the line of least resistance. . . . We are coming to the point now in psychology at which we cannot look upon states of feeling as *causes* of action." Cf. also S. J. Holmes, *op. cit.*, passim.

[3] I refer to the account offered by Watson, *op. cit. sup;* H. A. Carr, Principles of Selection in Animal Learning, *Psychol. Rev.*, 1914, **21**; M. Meyer, 'Fundamental Laws of Human Behavior.'

[4] First published in 1901. The view as expounded here is developed more explicitly in the second edition, 1915.

objects of the second sort will have lost their former power to excite, through its now being neutralized or overbalanced by the associated inhibitory excitement.[1]

But this is evidently an imperfect account of the matter. The reaction of rejection is no more inhibitory to that of pecking than is the reaction of swallowing. What the chick learns is not the simultaneous performance of two confirmatory acts, but a sequence of inhibitory acts. Why, then, should the chick not learn to pick up and reject, rather than to pick up and swallow? Or why should it acquire either of these habits to the exclusion of the other? To explain the prepotence of one of these sequences over the other, it is evident that what we need is some original connection uniting the pecking reaction with the swallowing reaction, but not with the rejecting reaction. The connection cannot be one of simultaneous compatibility or incompatibility between the reactions as such. It must be a connection between successive movements. We must conceive the pecking reaction as part of a total response of which the swallowing is the complementary *after*-part. Swallowing must be regarded as a prolongation or completion of the pecking response, in the direction of first intent, whereas rejection is an interruption or reversal of it. We must say that a pecking chick is an eating chick; and that it is this total eating response which selects the objects habitually to be pecked at. In other words, in this case *eligible* means *edible*.

It is not necessary to suppose that any mysterious psychic force is at work. If pecking is a part of eating, then it will be accompanied by the partial excitement of the swallowing reaction,—by a 'getting ready' to swallow. This anticipatory reaction will be brought to completion by certain stimuli such as the yolk of egg, inhibited by others, such as the orange peel. In the future the former will awaken these anticipatory reactions more strongly, and so reinforce the pecking reaction; whereas the latter will partially excite the rejecting reflex, which will diminish the force of the pecking reaction by inhibiting the anticipatory swallowing

[1] Hobhouse, *op. cit.*, second edition, pp. 118, 121.

with which that reaction is normally correlated. This would not be the case unless swallowing were in some sense the natural sequel to pecking, unless the two were somehow already organized in the animal's nervous and muscular structure.

This view of the matter finds expression in some of Hobhouse's statements, as when he speaks of 'confirmatory movement tending to prolong the reaction, or *carry it out* strenuously *to its final development*.' Similarly, he speaks of the result of the first reaction as following 'closely enough to impinge upon and so confirm or inhibit *the conational impulse by which that reaction is initiated and sustained*.' [1]

Professor Holmes, who follows Hobhouse in the main, states the view as follows: "Pecking and swallowing form the normal elements of a chain reflex; when one part of the structure concerned is excited it tends to increase the tonus of the associated parts and thus reinforce the original response." [2] But while this statement recognizes the necessity of presupposing some original connection between the first reaction and the 'successful' reaction which is selected, that connection still remains too external. He finds it necessary to suppose that the second reaction somehow modifies the first owing merely to a 'close temporal relation'; as though, apparently, the second reacted upon the first by a sort of back set. He fails here, I think, sufficiently to recognize that the two responses are really parts of one response. This appears also in his allusions to instinct. "A response," he says, "which results in setting into action a strong instinctive proclivity is reinforced or inhibited, as the case may be, according to its congruity or incongruity with the proclivity thus aroused. . . . Ordinarily a response *A* that is followed closely by an instinctive reaction *B* involving the liberation of a considerable amount of energy, is reinforced, probably as a result of the influence of this energy on the nervous connections simultaneously excited by the response *B*." [3] He should, it appears to me, have included the first

[1] *Op. cit.*, pp. 120, 123. The italics are mine.
[2] *Op. cit.*, pp. 135–136.
[3] 'Studies in Animal Behavior,' pp. 148–149.

response within the instinct or proclivity, and regarded the first and second responses as congruent parts of it. The instinct dominates the performance throughout, initiating it, and selecting the congruent sequel, the whole instinctive performance being under way from the beginning, in its tentative, as well as in its successful and habitual stages.

The most explicit statement of the view is to be found in a recent article by J. Peterson.[1] This writer formulates what he calls 'the *principle of completeness of response*.' Learning processes 'involve more or less complex *attitudes*.' "The total reaction is in a degree incomplete, tentative. It is conditioned by various muscular 'sets,' or tensions, partial responses to immediately distracting stimuli, which cannot relax wholly until relief is obtained from confinement, or food is reached." [2] In other words, there is a 'general' or 'main' response, marked by tension and nascent activity. This tension is relieved only when 'the act as a whole is complete.' "There is . . . a continuous overlapping of responses, some of which are in opposition while others are mutually helpful and *serve to the main response as additional stimuli, the latter leading to a more easy and complete expression*." [3]

If the above analysis is substantially correct, we find in the learning process a species of behavior that gives an empirical and objective meaning to the teleological vocabulary. A docile or corrigible organism is acting under the influence of a controlling impulsion which selects and acquires the specific instrumentalities through which it may be realized and completed. In so far as its behavior is thus determined, the organism may be said to be acting in the interested or purposive manner named for the general impulsion. In so

[1] Referred to above, p. 8, note. Cf. also Stevenson Smith, *Jour. Comp. Neur. and Psychol.*, 1908, 18.

[2] *Op. cit.*, p. 158.

[3] *Op. cit.*, pp. 156, 159. The writer goes on to say: "In our observation of animal behavior we have been too much interested in the principal response of the animal and have neglected to note sufficiently all the subordinate attitudes and responses." I should say rather that there had been too much neglect of the principle of subordination itself, whether through attention to the constituent reflexes or to the total performance.

far as the organism adopts a specific course of action because the expectation or preparatory response which it arouses coincides with that of the general controlling propensity, it may be said to act *for the sake* of the latter, or *in order to* realize it.

I have not thought it necessary for present purposes to discuss the extent to which purposive or teleological processes are hereditary. Theoretically instincts are supposed, like reflexes, to be altogether hereditary. But there are no clear cases of instinct of which this is true. Such responses as fear, pugnacity and the like are formed as they go, out of reflexes which are themselves highly modifiable. That hereditary structure defines the range of possibilities from which choice is made is doubtless true; but choice *is* made through the results of the organism's present experience. Similarly, the appetites signify certain hereditary and recurrent impulses which set the organism to acting until a specific relief is obtained; but just how any individual organism under given circumstances shall satisfy its craving for food or sexual intercourse is ordinarily determined by the results of tentative movements. It is this margin of modifiability, be it great or small, to which we must look for the factor of purpose. This interpretation will carry purposiveness far down in the phylogenetic and ontogenetic scale, but I cannot see that that argues against it.

It is essential that the action should be thus determined by its relation of prospective congruence with a controlling propensity which is both prior and more general. In other words the purposiveness is to be seen neither in the higher nor in the lower propensity regarded by itself, but in the interrelation of the two. The peculiar character of action in this case lies not in its merely having the character of complementary adjustment, but in its multiple and ulterior determination. We may now say of the 'successful' act, not merely that it is successful, in the sense, for example, of securing the food which the organism needs, but that it occurs *because it is successful.* Its being complementary to the environment, in a certain respect, accounts for its performance. It has actually been selected on this account.

Once the lesson is learned, the force of habit begins to operate; but behind the habit lies the higher propensity which has selected it, and which still exercises a certain control upon it. The lower propensity is always on trial or sufferance, so to speak; the acts which it determines occur only so long as they agree with the higher propensity. An organism in so far as acting purposively is always docile; while a purpose is always capable of inciting to new and untried efforts, and of exercising a selective function with reference to the tentative acts which it instigates. In other words, it is essential to behavior of this type that the higher propensity should be alive, or actually at work in the organism. At the moment when the habit becomes independent of this higher control, it becomes automatic. It can no longer be said to operate owing to its success. Its success accounts for its genesis; but the habit has now been weaned and is no longer answerable to the conditions with which it had to comply at birth and during its period of dependence. Herein lies the essential difference between the learning process and natural selection. According to the latter principle the character of hereditary structure is accounted for by its having been adaptive in ancestral organisms. But in so far as natural selection alone is predicated, the action of the organism cannot be said to be presently governed by this condition. In so far as its hereditary structure ceases to be adaptive through a change in the environment, the organism tends to weakness or death, and is not likely to transmit its traits; but it goes on with its maladjustment none the less. There is nothing in its constitution to forbid, or to prompt to new and more successful modes of adjustment. The organism cannot in this case be said to be *trying* to cope with its environment.

When a lesson has been learned, and a new habit established, it is quite possible that this in turn should become a higher propensity. If the kitten should be excited to effort by the mere appearance of a button in a vertical position; if these efforts should continue until a way was hit upon to turn it horizontally; and if the random efforts should then

be replaced by a stable propensity to perform the successful act, then we could say that the kitten was *trying to turn the button*, or that what it did was due to its producing the effect of a button in the horizontal position.

The variability of purposive action is not an accidental feature. What is required, as I have pointed out, is to be able to say that an act's performance is somehow conditioned by its having or promising a certain result. In order that this shall be possible, it is necessary that acts of the preferred sort should be actually selected from a larger class of acts. The rejected acts must actually occur so that the preference or selection may be manifested. An organism which reacted in one way, having a certain result, could not be said to prefer that way or to choose that result simply because other ways, having other results, were *conceivable*. It is necessary that these other ways should occur, in order to provoke the organism to the rejection of them. In order that an organism may be said to act in a certain way *because* of a certain result, it is necessary that acts, proving themselves to have a certain result, should derive a tendency to occur from this fact; and that other acts, proving not to have the result, should derive from that fact a tendency to be excluded. It is necessary that acts of the eligible type and of the ineligible type should occur *tentatively*, and then take on a stable or dispositional character according to the result. The occurrence of the experimental acts which this operation predicates is provided for only by the variability of behavior under the excitement of a deep-seated and persistent propensity.

We may consider this variability under three aspects. (1) In the first place, *the behavior is variable in respect of the response itself*. This is what Professor Thorndike terms 'the fact of multiple response to the same external situation,' and which he says pervades 'at least nine tenths of animal and human learning.' [1] In other words, it is not the constancy of the organism's behavior that is here remarkable, but its resourcefulness. Many acts are called, though few

[1] *Op. cit.*, p. 12.

be chosen. It is a well-known fact that in the ascending scale of animal development this variability of response becomes more and more pronounced. In man, and in those men whose conduct would be said to exhibit a relatively high degree of purposefulness or intelligence, this resourcefulness becomes well-nigh inexhaustible. The 'fertility' of mind which characterizes invention, is the same thing in principle, the difference lying in the fact that the lessons already learned, and the process of inference, render it unnecessary that more than a few of the responses should be carried through in order that their result should be proved.

(2) In the second place, *the behavior of learning is variable in respect of the feature of the environment to which the organism responds.* This is what Professor Thorndike calls the 'Law of Partial Activity,' according to which "a part or element or aspect of a situation may be prepotent in causing response, and may have responses bound more or less exclusively to it regardless of some or all of its accompaniments."[1] The situation in which the organism is operating is highly complex, and contains many features to which the organism's sensory apparatus qualifies it to respond. In the case of the kitten already described there are the button, the bars, the sides, the experimenter himself, etc., to one or another of which attention may be shifted. The possibilities are proportional to the mobility of attention, to the fineness of discrimination, and to the variety and delicacy of sensory capacity. Among the stimuli in the case of the kitten is the smell of the food itself. But the essential feature of the process as one of trying and learning, is that the simple reflex excited by this stimulus gives place momentarily to other reflexes excited by other stimuli. Thus though the first impulse be one of motion toward the food, this gives way presently to the impulse to push away the bars with which the animal comes into contact, and which stimulate its tactual or muscular senses.

(3) Being thus excited to various responses by the various aspects of the situation, acts occur which have different

[1] *Op. cit.*, p. 14.

results. In other words, the *consequences of action are variable.* Some of these consequences or end-results are of the character defined by the higher or controlling propensity, some are not. Thus $e_1 + r_1 = a$, $e_1 + r_2 = b$, $e_2 + r_2 = c$, $e_3 + r_3 = m$, etc. But of these acts that which results in m is unique. It terminates the process of trying, and tends to recur more promptly when that process is renewed. When $e_3 + r_3 = m$ does thus recur, bearing the peculiar relation which it does to the general set $E + R = M$, it may be said to be performed for the sake of M. This peculiar relation to $E + R = M$ is both a matter of past history and also of future tendency. On the one hand, $e_3 + r_3 = m$ is selected, and brings the propensity $E + R + M$ to rest, because m has proved to be a case of M. On the other hand, $e_3 + r_3 = m$ owes its own stability as a propensity to the persistence of the propensity $E + R + M$, and to m's continuing to be the result of $e_3 + r_3$.

Although the matter cannot be fully dealt with here, it is desirable to make at least a brief reference to what would usually be regarded as the mental aspect of this process. But believing as I do that to explain this process by a reference to what is commonly regarded as consciousness would be to commit the fallacy of *obscurum per obscurius*, I shall rather attempt to obtain light on the problem of consciousness by reference to certain aspects of this process.

Whatever excites endeavor or conation in the behavioristic sense already described, inaugurates the very state in which that endeavor is to terminate. As Professor Warren has pointed out, although "the dog certainly does not eat the rabbit before he catches it, . . . nevertheless, the act of eating is begun before the appropriate food stimulus appears."[1] As even Professor Warren appears not sufficiently to recognize, the process of eating is actually commenced in so far as the dog is trying to eat the rabbit. I refer, of course, to the secretion of gastric juices, the incipient muscular contractions required for seizing and tearing the prey, etc. In this sense what the animal is trying to do may be said to be

[1] H. C. Warren, A Study of Purpose, *J. of Phil., Psychol., etc.*, 1916, 23.

anticipated; or we may say that the animal expects it momentarily. Where this is the case we may say that the appearance of the prey is a sign of the eating, or awakens the expectation of it. But expectation in this sense may be awakened not merely by the appearance of the object which is a constituent of the end-result; it may be awakened by a means to that end-result; by the stimulus which one has learned to respond to successfully, or by the response which has proved successful. Where, then, any of these factors releases in any measure that response which is a part of the consummation of the endeavor, or causes the organism to behave as though the endeavor were consummated, we may speak of *conscious* purpose. The factor which so operates means satisfaction to come, it promises something, arouses expectation. This, I should say, is what we mean by anything's being an object of consciousness.

To make the matter clearer it is necessary to introduce the case of simple awareness or cognitive response. In this case the endeavor is that the stimulus should more effectively stimulate. The organism responds by a better adjustment to the action of the stimulus, by turning the head, focusing, cutting off inhibiting or disturbing stimuli; in short, by looking, listening, touching, sniffing and the like. This attentive set is like any other except in that it usually accompanies the others and facilitates them. But it is quite capable of absorbing the energies of the organism for a time. Then when it comes to rest, one can be said to have one's curiosity satisfied, and to have learned how to 'see' what one was trying to see. The attentive act is an anticipation of future stimulation, and whatever excites it is object of consciousness in the sense just described.

One further consideration. It is characteristic of human conduct that *it is guided by 'ideas.'* In the essential sense all signs, all stimuli that excite anticipation or arouse expectation, may be called ideas. The English philosophers, therefore, were not so far wrong as is usually supposed in identifying ideas with mental contents. But it is possible, if one prefers, to restrict the terms to arbitrary signs. By what Professor

Thorndike calls 'Associative Shifting' it is possible to 'get *any response of which a learner is capable* associated with *any situation to which he is sensitive*.'[1] Thus the arbitrary combination of sounds which constitute the spoken word 'food' may excite the food-getting reflex, and similarly the expectation of food. It is probable, to my mind, that the primary function of a word is to excite the attentive act which is appropriate to a given stimulus. In other words, when the word 'food' is spoken, the organism is beginning to see food, and then over and above this to deal with it in the manner determined by the food-eating propensity. Certainly in the case of man, to 'understand' a word, is to be put in readiness for certain stimuli.

Or it is possible, if one prefers, to restrict the term idea to centrally stimulated signs. Such signs may consist of visual images resembling the object signified; or they may, as in the case of vocal-motor images, be quite arbitrary. The important thing is that they should excite anticipation or arouse expectation. This is sufficient to make them ideas. That they should be centrally stimulated is practically important, in that it makes it possible for the individual to proceed to act upon any propensity, independently of present circumstances in the environment. Thus one may in this way seek food, whether or not either food or anything to suggest it happens to stimulate the organism from without.

There is one further consideration which is too important to omit even in this hasty summary. Endeavor, especially in the case of man, is often terminated, not by the end result toward which the organism is set, but by a belief which may, as it happens, be mistaken. In such cases, we must say that the real object of endeavor is a situation which in turn creates certain new expectations. These expectations may in any given case be doomed to disappointment. Thus the animal may store away against the winter's famine what he takes to be food; whereas the objects collected may as a matter of fact not be edible, being made to deceive by their resemblance to familiar forms of food. We must then say

[1] *Op. cit.*, p. 15.

that the endeavor of the animal was to reach a condition in which there is an anticipation of eating; the anticipation being excited, the endeavor comes to rest. The successful means employed to collect the spurious food is learned, and tends to become stable, just as if it were really edible. We must therefore predicate a set that is satisfied independently of the actual edibility of the objects in question. And this satisfaction must be held to consist in the animal's expectation. It is upon this model, I believe, that we must construct our account of the higher purposive processes of man, in which the purpose is none the less present even when misled and founded on error.

In the present paper I have put together certain notions of the learning process in the belief that they afford an account of behavior to which the term purpose is properly and significantly applicable, but without implying any factor out of keeping with the most rigorous scientific method. The important feature of docility is not adaptation to the environment, but the acquiring of specific modes of adaptation, and performance determined by the experience of adaptation. It appears to be necessary to predicate two springs of action in the docile organism: (1) the more deep-seated, sustained and general propensity, which accounts for the increased reactivity called 'trying' and which prescribes when this shall be brought to rest; (2) the more superficial, transitory, and specific propensities, which are rendered hyperexcitable by the former, but are ordinarily released by sense stimuli. The former we may call the selective, dominant or higher propensity, and the latter the tentative, subordinate or lower propensity. That one among the tentative propensities which is selected, and which we may term the eligible propensity, is one which confirms, facilitates and amplifies the selective propensity. When a tentative propensity has thus proved congruent with a selective propensity, then in future when that same selective propensity is moving the organism, or is dominant, then the tentative propensity in question has a prepotence over others, past and present, because of the greater compatibility between the expectation which it now

arouses and the general direction or set of the dominant propensity. Expectation or set can, I think, be construed physiologically in terms of nascent adjustments. Action so performed can fairly be said to be performed owing to the agreement of its promise or prospective sequel with that of a more fundamental but more flexible propensity, which in this relation and function may be called a purpose. Action determined by an eligible propensity, is done 'on purpose.' Docility thus construed requires that the behavior of the organism shall be variable in all three of the aspects into which it can be analyzed: namely, feature of the environment attended and responded to, physical movement, and effect. Purposiveness thus appears in life *pari passu* with variability or modifiability of behavior. Finally, the same mechanism which is implied in the learning process when so construed, will serve also as a crude account of the so-called 'higher processes' in which one solves a problem, or conceives and executes a plan.

THE PERCEPTION OF FORCE[1]

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I. Introduction

(*a*) *Statement of the Problem.*—If an individual is asked to pull a spring to a point where the tension will amount to twenty pounds he will very probably make a large error. If he is permitted to repeat the pull after being told the amount and direction of his error he will probably either reverse the direction of the error or approach nearer to the required twenty pounds. Succeeding corrections and trials will enable him to reduce the size of his errors; and they will, if tabulated, give an approximation to the normal probability curve with the twenty pounds as a mean.[2] To delineate the mechanism which makes such learning possible and to describe the modifications involved would be no simple task; evidence for which is given in the antagonistic results of previous investigations on this subject.[3]

Disagreement in the findings of science is due either to faulty experimental control, to incorrect interpretation of results, or to both; hence when one meets contradiction in the results of different experiments it is first necessary to see whether the conclusions as stated by the investigators necessarily follow from their experimental data, and if the trouble

[1] The experiment of which this article is a report was made possible through a Cutting Travelling Fellowship granted the writer by the trustees of Columbia University in the spring of 1916. The apparatus was made and the experimenting largely done in the psychological laboratory of the Johns Hopkins University under the supervision and with the generous assistance of Professor John B. Watson. The experimenting was completed in the Princeton psychological laboratory and the data put in shape while the writer was instructor in psychology in Princeton University.

[2] Fullerton, G. S., and Cattell, J. McK., 'On the Perception of Small Differences,' Publ. of the Univ. of Penna. Philos. Series, 1892, No. 2.

[3] Sherrington, C. S., 'The Muscular Sense,' Shäfer's 'Text-book of Physiology,' 1002–1025. James, Wm., 'Principles of Psychology,' Vol. II., 486–520. Woodworth, R. S., 'Le Mouvement.'

cannot be located there to repeat and modify the experimental conditions in order to obtain supplementary information on the subject. Our problem will therefore be to investigate the nature of the conflicts in previous experiments on the perception of force and then after we have clearly formulated the point at issue to attempt to supply data based on experimental modifications of such a nature as to give them strong evidential character.

(*b*) *Some of the Points at Issue.*—In connection with this problem one fact that previous investigations have demonstrated is that the 'sense of tension,' 'sense of resistance,' or 'sense of force' is a very complex affair. If there were a unitary sensation of innervation accompanying the discharge of motor impulses; if there were a simple sensation of tension, of energy expended, or of muscle change accompanying movements of the body; if extent or time were the fundamental thing in the perception of movement;—if any one of these were the irreducible element, no such discordant results as have been obtained would have confused investigators and no doubt the problem would long ago have been settled. Different investigators have as the result of different experiments argued for the importance of each of these factors in the production and perception of movement, and each new experiment seems to bring to light some new phenomenon which contradicts what some previous investigator has found. One factor among those mentioned that seems to have been discredited by recent experiments is the sense of innervation.[1] At least there are such strong arguments against it that we need not consider it here. If then kinesthesis depends wholly upon afferent impulses, the question is left as to whether it is determined principally by the quality or intensity of these impulses or by their spatial or temporal relations, or by all these combined. Under certain conditions it has been shown that one judges force by movement speeds,[2] in other conditions extent has been shown to be important, while in others the criterion is the latent time required to overcome the

[1] *Op. cit.*

[2] Müller and Schumann, *Arch. f. d. ges. Physiol.*, 1889, 45.

opposing resistance.[1] Clinical experiments on the effect of anesthetizing the superficial areas on the perception of lifted weights have given discordant results.[2]

Fullerton and Cattell give three arguments against the contention of Müller and Schumann that force is judged by speed. (1) The force of a movement can be judged better than its time. (2) The judgment of time follows Weber's law more nearly than the judgment of force. (3) When the rate is altered so that the one is lifted four times as rapidly as the other, either by being lifted higher in the same time or the same distance more quickly, the probable error is not increased. They state that this latter unexpected result proves conclusively that we do not judge of difference in weights by the rate at which they are lifted. They are of the opinion that lifted weights are judged by a combination of skin, pressure and muscle sensations.[3]

(*c*) *Previous Work Leading to the Present Paper*.—The thesis which led to the experiments to be reported in this paper was that the perception of force is very crude and its seeming accuracy in certain instances depends upon the adoption of secondary criteria. We will first refer very briefly to the facts which led to this thesis, show how the use of secondary criteria could have entered into Fullerton and Cattell's experiments on the perception of force, and this will bring us to an explanation of the method and plan of our experiments.

It has been found that when a person is told to raise a weight with all the force he can, if the weight is changed he will tend to pull varying masses with the same speed, which means that the force of the muscular contraction must vary with the different weights.[4] The time required for this adjustment of force has been found to be fifty sigma or less, in no case more than 100 sigma. It must therefore be either a reflex or a local muscular phenomenon. Further, when the

[1] See discussion of the experiments of Jacobi in Woodworth's 'Le Mouvement.'

[2] Sherrington, *op. cit.*

[3] *Op. cit.*

[4] Morgan, J. J. B., 'The Overcoming of Distraction and Other Resistances,' *Arch. of Psychol.*, No. 35, 1916.

subject is told to pull several weights with the same force he can make but crudely the time adjustment that is necessary.[1]

These facts show that if one has any elementary sensation of force it is not the same thing we have to deal with in physics. In physics, if we have a certain force acting against a certain mass we will get a definite acceleration, the force being equal to the mass multiplied by the acceleration. If the mass is changed and the force kept the same the acceleration can be determined from the equation and is found to be borne out by experiment. If we tell a subject to set his own force in pulling a certain weight we can measure the acceleration and can determine the amount of physical force used. If we change the mass and tell him to use the same force as before we find that acceleration is but little changed but that the resultant physical force is. Certainly this shows that the subject has no unitary sensation of physical force; or, if he has, he is grossly ignorant of its relation to acceleration. The force of a spring, of an explosion, or of gravity is vastly more accurate in its adjustments than the human muscle.

Over against these facts stand the experiments of Fullerton and Cattell which showed that force can be judged more accurately than time, although somewhat less accurately than extent. In all their experiments, except in one with extent, they used the following procedure: The subject was given a practice series in which to learn the standard magnitude, whether extent, time, or force. After the practice series the movements were made in pairs. The first of each pair was an attempt to approximate the standard magnitude, the second of the pair was an attempt to equal the first. In this way the subject made his own standard which he used for comparison. The average errors were taken from the differences between the two movements of the pairs. The subject was told at the end of each ten pairs how much he was above or below the standard magnitude, and thus could

[1] *Ibid.*, 'The Speed and Accuracy of Motor Adjustments,' J. of Exper. Psychol., June, 1917.

attempt to make the necessary correction. In the experiments on extent they used standard magnitudes of 100, 300, 500 and 700 mm.; in the experiments on time they had the subjects make a 50 cm. movement in minimum time, in 250 sigma, in 500 sigma and 1,000 sigma; in the experiments on force the subjects endeavored to pull the handle of a spring dynamometer with a force of 2, 4, 8 and 16 kg. In the experiments on extent no record was taken of the time of the movements, in the experiments on force no record was taken of the time, and in all except the 16 kg. pull the force was a direct function of the extent of the pull. For these reasons we believe that a comparison of the relative accuracy of the perception of time, force and extent cannot be derived from their experiments.

Since we are mainly interested in the force of movements we will study a little more closely their experiments on this phase of the subject. The dynamometer they used moved 6.4 mm. for every kg. up to 10 kg., for pulls of 10 kg. or more they changed the apparatus so that movement began at 10 kg. and for each kg. above 10 the handle moved 6.4 mm. This means that in pulling a standard of 2,000 grams the subject had a standard extent of 12.8 mm. to strive for; in pulling a standard of 4,000 grams he had an extent standard of 25.6 mm.; in pulling a standard of 8,000 grams he had an extent standard of 51.2 mm.; and, in pulling a standard of 16,000 grams he had an extent standard of 38.4 mm.

We have no way of telling what the variable errors of Fullerton and Cattell's subjects would have been had they been given extent standards of these values. If they had experimented with such extent standards we could compare them with the extent errors when they used 2, 4, 8 and 16 kilograms as standards and might thus ascertain whether the force pulls were influenced by the extent of the movements made.

It might nevertheless be interesting to determine what the extent errors for the 12.8 mm., 25.6 mm., 51.2 mm. and 38.4 mm. standards would have been on the basis of their extent experiments with 100, 300, 500 and 700 mm. standards,

if calculated by Weber's or Cattell's psycho-physical laws.[1] The ratio of error for the 100 mm. movement was for their subject F. one to 18.9.[2] If this ratio were to hold according to Weber's law this subject would have scored a variable error of .676 mm. with the standard of 51.2 mm., 1.42 mm. with the standard of 25.6 mm., 2.84 mm. with the standard of 51.2 mm. and 2.13 mm. with the standard of 38.4 mm. If on the other hand Cattell's square root law held good this subject's variable errors for the several linear magnitudes would have been respectively 1.90, 2.69, 3.79 and 3.29 mm. Now in their force experiments a pull of one mm. on the dynamometer which they used equalled nearly 156 grams. If then we transmute the linear errors into gram errors we will get the result shown in Table I. By comparing these

Table I

The Relation between the Variable Errors Obtained from Subject *F* in Fullerton and Cattell's Force Experiments and the Variable Errors as Computed for Movements of Similar Extent from their Experiments on Extent

Force Standards / Extent Standards	2,000 / 12.8	4,000 / 25.6	8,000 / 51.2	16,000 Grams / 38.4 Mm.
V. E. Subject *F* in force exp.	183	280	373	434 grams
V. E. Weber's law from extent exp.	105	221	443	332 grams
V. E. Cattell's law from extent exp.	296	420	591	513 grams

two sets of computed variable errors with the actual variable errors made by this same subject in the force experiment it will be seen that for the most part the actual force errors fall between the force errors computed from the 100 mm. standard by the two methods. This is evidence enough at least to suggest to one the hypothesis that the force pulls were in large part guided by extent, and Fullerton and Cattell could have secured the results they did if their subjects possessed only a very crude perception of force as such. Theoretically this removes the disparity between the

[1] Cattell, J. McK., 'On Errors of Observation,' *Am. Jour. of Psychol.*, 1893, *5*, 285–293.

[2] Fullerton and Cattell, 'On the Perception of Small Differences,' Univ. of Penna. Publ. Philos. Series, 1892, p. 48.

results of our experiments and those of Fullerton and Cattell. We found that the force a person exerted was determined by the resistance encountered and that the subjects could not consciously adjust the speed of their movements so as to use the same force in moving different masses. Fullerton and Cattell found that force could be judged better than time; but, as their subjects may have been judging by extent, any comparison of force with time or extent is ruled out. It remains for experiment to show whether this hypothesis, that the perception of force is largely dependent on other factors, can be proven and it is this we have attempted to do in the experiment we are about to report.

An observation made by Professor Woodworth has a bearing on the problem.[1] He has shown that if the several factors that enter into a perception are perfectly correlated the variable error will increase in direct proportion to the stimulus (Weber's law), while if the factors are not at all correlated but are operative in a purely chance way the variable error will increase in proportion to the square root of the stimulus (Cattell's law). Where there is some correlation the error of observation will fall between the error required by the two laws. Now if all the causal factors of any perception are directly correlated, by the very nature of the case these factors must be relatively few, for by chance we mean a number of factors working indiscriminately, hence the larger the number of factors the greater the likelihood of a pure chance series. In Fullerton and Cattell's experiments the variable error in time followed Weber's law very closely, the variable error in extent followed Cattell's square root law, and the variable error of force fell between. This would indicate that the perception of time is relatively simple compared with that of extent or force regardless of their relative accuracy, and we believe our experiments will show the complex nature of the perception of force and extent. This lack of correlation between the factors controlling the force of movements may account for the conflicting results

[1] Professor Cattell's Psychophysical Contributions, in the Psychological Researches of James McKeen Cattell, *Arch. of Psychol.*, 1914, No. 30, pp. 70–72.

obtained in experiments on the perception of force. Müller and Schumann found that under certain conditions the perception of lifted weights correlated with the speed with which they were lifted. Cattell varied the speed and found perception as accurate as before. If weights are judged by a number of non-correlated factors, a subject could readily shift from one basis of judgment to another. We feel that in the study of the subject this fact should receive strong emphasis. If we are studying a form of perception which depends upon, let us say, five correlated factors and we experimentally interfere with one factor, the total perception will be changed more radically than would be the case if we were to interfere with one element in a perception that depended on five factors operating in a purely chance manner. Translated into the terms of our problem this would mean that, if the perception of force depends on several non-correlated factors, under normal conditions the subject will judge the force of his movements; or, in objective terms, the error in his movements will be determined by all, several or perhaps only one of these factors. Suppose his force movements are determined largely by time; then, if time is varied he might shift to extent. If extent were varied or eliminated he might revert back to time unless it were still controlled. If both time and extent were eliminated, skin and muscle sensations might be called upon to bear the larger part in the force control and judgment. If, therefore, force depends upon one factor the control of this would perfectly control the error of a force movement. If it depends on several partially correlated factors the relative importance of the different factors could be determined experimentally. If it depends on several chance factors the only way to change the error and judgment of force would be to have adequate control of all the factors. We believe our results will show that Woodworth was right in his theory, and that force depends on a number of partially correlated factors. We may also be able to show the relative importance of some of them.

If it is possible to show that this is the case it may shed

some light on what we mean by adaptation. It is possibly nothing more than an evidence of the multiplicity of causes underlying activity of any sort. If the causes of an act are few or closely correlated, adaptation will be less complete than if the causes are numerous and related only in a random way.

II. General Plan of the Experiment

To arrange an experimental procedure which would show what determines the control and judgment of the force of movements was our problem. Three major experimental variations were used, the same general procedure being used in all. The general procedure was to inform the subject of the task; that is, whether to attempt to make a movement of a certain length or to pull with a certain force. Having received his instructions he was given twenty-five practice trials, the amount and direction of his error being given after each trial. After this practice series the movements were made in pairs. In the first of each pair the subject tried to produce the standard, while in the second he tried to reproduce the first. This is the procedure devised by Cattell and Fullerton and it permits the subject to make his own standard for each movement and gives a much more accurate record of the subject's perception and control than if the arbitrary standard was used as a base from which to compute the errors. After the second movement of each pair the the subject gave a judgment as to the direction of the difference between the two. After he had given this report the experimenter told him the direction and amount of error of the second of the pair when compared with the arbitrary standard given at the beginning. He was thus enabled to make an intelligent effort to correct the first movement of the next pair which we will call the standard. He was not told whether his judgment as to the direction of the error between the two pulls was correct or not. Fifty pairs in addition to the twenty-five practice movements constituted one experimental sitting.

The experimental variations were as follows:

1. The subject was instructed to pull with a certain force

and corrections were given him in terms of the number of grams too heavy or too light. In all these experiments the extent of the pulls was the same for each standard force from 2 to 16 kg. Time records were taken for each pull, the chronoscope starting when the pull began and stopping the instant the return stroke was initiated.

2. With a change in the arrangement of springs on the dynamometer between experimental sittings the subject was given instructions to pull a certain distance and corrections were given in terms of millimeters too long or short. Time records were taken for each pull, as in the previous procedure.

3. The subject held his arm as nearly stationary as possible and the experimenter increased the tension, the subject calling out when he judged that the tension had reached the required amount.

An experimental sitting consisted of 25 practice pulls followed by 100 paired pulls with one set of springs. A set of experiments included experiments with 2, 4, 6, 8, 10, 12, 14 and 16 springs. Including the 3,200 practice pulls the experiments to be reported are based upon 16,000 pulls and 6,400 judgments.

The sequence of the experiments was varied with the different subjects so that when averaged together practice effect was eliminated in the average scores. Subjects *A*, *B*, *C* and *D* had an entirely different order from *E*, *F*, *G* and *H*. In addition the sequence for *A* and *B* was exactly the reverse of that for *C* and *D*, and that for *E* and *H* the exact reverse of that for *F* and *G*.

The subjects in the experiment ranged from those who were highly trained in experimental psychology and laboratory procedure to those distinctly untrained. We could find no tendency for the untrained to differ specifically from the trained.

III. Description of Apparatus

The dynamometer consisted of a handle connected to a rod which ran on roller bearings so as to minimize friction. From this rod projected a smaller rod at right angles which moved an indicator before it as it made the forward stroke.

On the return stroke this rod left the indicator, thus giving the experimenter an opportunity to read the extent of the movement from a millimeter scale which was attached to the

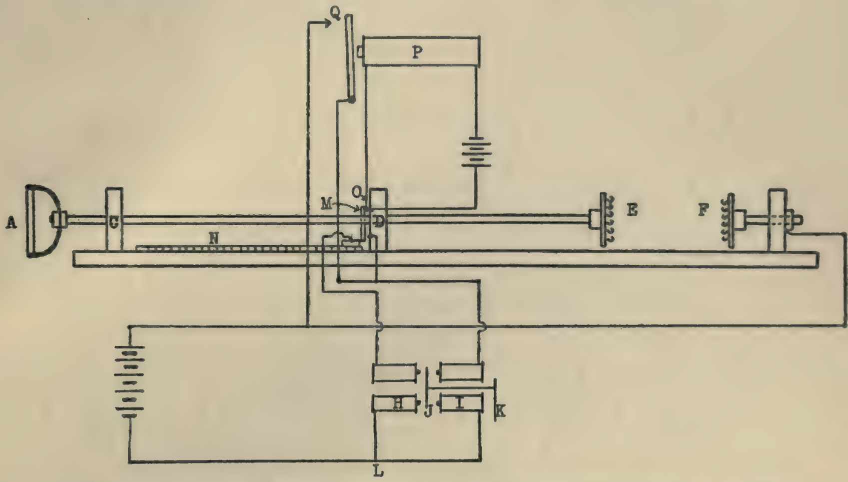

FIG. 1. Outline of Dynamometer and Dunlap Chronoscope with Electrical Connections. *A* is the handle of the dynamometer which connects with the rod *B* which in responding to pulls on the handle works on roller bearings at *C* and *D*. *E* and *F* are plates each equipped with 16 hooks upon which may be fastened 16 springs in parallel. *H* and *I* are the two sets of magnets in the Dunlap chronoscope. The magnets *H* are connected to the armature of a synchronous motor which is in motion throughout the experiment. When everything is set for a pull the rod at *M* is connected both through the magnets *I* by a contact which breaks as soon as the handle moves forward and through the magnets *H* by a contact with the marker which rides on the scale *N*. At *L* is inserted a Dunlap key (omitted in the drawing for the sake of clearness) which closes the contact through *I* an instant before it closes that through *H*, thus making certain that the armature *J* is against the stationary magnets *I*. The breaking of the contact through the magnet *I* at the beginning of a pull permits the armature to be pulled away from the stationary magnets *I* to the revolving magnets *H* and the indicator *K* begins to revolve. The beginning of a pull likewise breaks a contact at *O* which breaks an independent circuit through the relay *P*. The armature of the relay being released falls back and makes a contact at *Q* which reconnects the circuit through the magnets *I* of the chronoscope. The armature of the relay is so adjusted that the remaking of the current would not be strong enough to pull it up and so the breaking of the contact at *Q* is not accomplished until the experimenter pushes it up with his hand. This prevents any inadvertent starting of the chronoscope. The forward movement of the handle moves the indicator along the scale *N* and as soon as the return stroke is initiated the contact through it and the rod *M* through the revolving magnets of the chronoscope *H* is broken, thus allowing the magnets *I* to pull the armature *J* forward and stop the chronoscope.

dynamometer. After reading the indicator was returned to *O*. An electrical contact was broken when the arm left its back stop at the beginning of a pull and a second contact broken when the rod left the indicator at the beginning of the return stroke. These two break contacts operated the magnets of a Dunlap chronoscope. The details of the chronoscope connections and operation are shown in Fig. 1. The rod attached to the handle of the dynamometer had at its other end a plate upon which were 16 hooks. This plate faced another similar plate which was fixed to the other end of the dynamometer. Between each of these 16 pairs of hooks springs could be placed or removed as desired. They were however always used in pairs so as to prevent lateral torsion. This was accomplished by using together two springs on a line with the center of the rod and equidistant from it. The springs were as nearly alike as possible but were carefully calibrated and the variations that were found were taken into consideration in the records. An extension of 175 mm. required a pull of one kilogram on a single spring. By arranging, in different experiments, 2, 4, 6, 8, 10, 12, 14 and 16 springs in parallel we were enabled to use standards of 2, 4, 6, 8, 10, 12, 14 and 16 kilograms and in each case keep the extent of the movement the same.

In the experiment where the subject held his arm stationary a second handle was attached by a wire to the rear end of the dynamometer, which in this case was suspended from two standards and the movable end connected to a windlass which the experimenter operated to tighten the springs. The subject held the handle as near to an index point as possible throughout the experiment and consequently the dynamometer and the handle he held only moved with the waverings of his hand. In every case the dynamometer was screened from the subject, and when he made arm movements a screen was placed between his arm and body so that he could not observe his movements.

IV. Results

In the first experiment the subjects were required to pull 2, 4, 6, 8, 10, 12, 14 and 16 kilograms, a different force being

asked for at each experimental sitting, the sequence of the different forces being determined by chance. Records were taken of the extent error and the time of each pull. The force errors were later computed from the extent error. In this series four subjects *A*, *B*, *C* and *D* were used and their

Fig. 2.

force, extent and time errors are given in Tables II., III. and IV. In each of the tables four records are given for each subject for each set of springs used. The first is the average force, time or extent of the first pull of each pair, called the standard. The second score is the average error, that is, the average difference between the first and second pulls. The third score is the variable error showing the variability in the size of the error between the first and second pulls. The last record is the ratio of the variable error to the standard.

In this experiment the extent factor was the same with all the various forces used. The subject could pull the handle different distances, as there was nothing on the apparatus to prevent this (except that a stop was arranged so that he could not pull hard enough to damage the springs), but if he pulled the exact force standard the extent of the movement would be the same for each force standard. The time factor

was not controlled in any way, the subject being left free to pull as quickly or as slowly as he pleased.

Table II

Force Records in Mm. of 1,600 Experiments in which the Subjects Attempted to Produce Two Movements of Equal Force, and in which Corrections Were Given in Grams.

Set Standard......	2	4	6	8	10	12	14	16 Kg.
Subject A:								
Av. of standard pull....	1,980	3,910	5,766	7,970	9,893	11,906	13,923	15,744
A. D.........	*107*	*162*	*244*	*562*	*304*	*360*	*546*	*618*
Average error.	102	246	264	304	325	354	469	584
Variable error	64	138	162	188	210	234	245	344
Ratio V. E. to stand	*30.9*	*29.3*	*35.6*	*42.4*	*47.1*	*50.9*	*56.9*	*45.8*
Subject B:								
Av. of standard pull ...	2,020	4,126	6,012	8,008	9,885	11,460	14,091	15,984
A. D.........	*46*	*190*	*315*	*365*	*458*	*456*	*582*	*736*
Average error	134	232	330	388	650	666	560	824
Variable error	85	156	189	192	230	324	329	496
Ratio V. E. to stand......	*23.8*	*26.4*	*31.8*	*42.1*	*43.0*	*35.4*	*42.8*	*32.2*
Subject C:								
Av. of standard pull....	1,943	3,946	5,902	7,972	10,099	11,974	13,780	15,887
A. D.........	*89*	*191*	*250*	*358*	*434*	*446*	*476*	*429*
Average error	117	251	395	310	479	492	549	507
Variable error	65	138	167	181	334	278	311	288
Ratio V. E. to stand......	*29.9*	*28.6*	*25.3*	*44.1*	*30.0*	*43*	*44.3*	*55.1*
Subject D:								
Av. of standard pull....	2,008	4,093	5,980	8,094	9,789	12,026	13,789	16,025
A. D.........	*73*	*154*	*197*	*237*	*360*	*367*	*591*	*464*
Average error	86	126	237	274	418	516	600	507
Variable error	60	78	145	156	266	396	358	294
Ratio V. E. to stand......	*33.4*	*63.3*	*41.2*	*51.8*	*36.8*	*30.4*	*38.4*	*54.5*
Averages:								
Standard pull	1,987.7	4,018.7	5,915	8,011	9,916	11,841	13,896	15,910
A. D.........	*78.7*	*174.2*	*251.5*	*380.5*	*389*	*407.3*	*548.7*	*561.7*
Average error	109.7	213.7	306.5	319	468	507	544.5	605.5
Variable error	68.5	127.5	165.7	179.2	260	308	310.7	355.5
Ratio.......	*29.0*	*36.9*	*33.5*	*45.1*	*39.2*	*39.9*	*45.6*	*46.9*

Before we examine the experimental results in detail it may be well to consider the relation of some of the factors involved and what certain results would mean. This will give a point to the presentation of the results, and give their exposition greater clearness. If the subject used extent as

a basis of control the extent errors should have been nearly the same in all cases, which would of course mean that the force errors would vary in conformity with Weber's law. If the time errors appear the same with all forces, it would indicate that time might help the subject to control force.

Table III

Extent Records in Grams of 1,600 Experiments in which the Subjects Attempted to Produce Two Movements of Equal Force, and in which Corrections Were Given Them in Grams.

Set Standard	2	4	6	8	10	12	14	16
Subject A:								
Av. of standard pull	171.92	168.54	164.2	171.26	169.86	174.44	173.92	172.8
A. D.	*10.68*	*8.08*	*8.12*	*14.04*	*6.08*	*6.0*	*7.8*	*7.72*
Average error	10.2	12.3	8.8	7.6	6.5	5.9	6.7	7.3
Variable error	6.4	6.9	5.4	4.7	4.2	3.9	3.5	4.3
Ratio V. E. to stand.	*26.9*	*24.4*	*30.4*	*36.4*	*40.4*	*44.7*	*49.6*	*40.2*
Subject B:								
Av. of standard pull	176.04	179.28	172.4	172.2	169.7	167.62	176.3	175.8
A. D.	*4.6*	*9.52*	*10.52*	*9.12*	*9.16*	*7.6*	*8.32*	*9.2*
Average error	13.4	11.6	11.0	9.7	13.0	11.1	8.0	10.3
Variable error	8.5	7.8	6.3	4.8	4.6	5.4	4.7	6.2
Ratio V. E. to stand.	*20.7*	*23.0*	*27.4*	*35.9*	*36.9*	*31.0*	*37.5*	*28.3*
Subject C:								
Av. of standard pull	168.28	170.28	168.74	171.3	173.98	175.56	171.86	174.54
A. D.	*8.92*	*9.56*	*8.32*	*8.96*	*8.68*	*7.44*	*6.80*	*5.36*
Average error	11.66	12.56	13.16	7.74	9.58	8.2	7.84	6.34
Variable error	6.52	6.92	5.56	4.52	6.68	4.64	4.44	3.6
Ratio V. E. to stand.	*25.8*	*24.7*	*30.3*	*38.0*	*25.9*	*37.8*	*38.7*	*48.5*
Subject D:								
Av. of standard pull	174.8	177.64	171.34	174.36	167.78	176.44	171.98	176.32
A. D.	*7.27*	*7.72*	*6.56*	*5.92*	*7.2*	*6.12*	*8.45*	*5.8*
Average error	8.64	6.3	7.9	6.86	8.36	8.6	8.56	6.34
Variable error	5.97	3.88	4.84	3.9	5.32	6.6	5.12	3.68
Ratio V. E. to stand.	*29.3*	*45.8*	*35.5*	*44.7*	*31.50*	*26.7*	*33.6*	*47.6*
Averages:								
Standard pull	172.6	173.93	169.17	172.28	170.33	173.51	173.51	174.86
A. D.	*7.87*	*8.72*	*8.38*	*9.51*	*7.78*	*6.79*	*7.84*	*7.02*
Average error	10.97	10.69	10.21	7.97	9.36	8.45	7.77	7.57
Variable error	6.85	6.37	5.52	4.48	5.20	5.13	4.44	4.44
Ratio	*25.68*	*29.47*	*30.9*	*38.75*	*33.67*	*35.05*	*39.85*	*41.15*

If the subject were ignorant of the relation of force and extent and could only judge time, the force and extent errors might show an interrelation and the time errors be the same.

In the actual experimental results the force errors (Table II.) increase with the size of the stimulus, not as rapidly as would be required by Weber's law and more rapidly than would be required by Cattell's square root law (see Fig. 3).

TABLE IV

TIME RECORDS IN SIGMA OF 1,600 EXPERIMENTS IN WHICH THE SUBJECTS ATTEMPTED TO PRODUCE TWO MOVEMENTS OF EQUAL FORCE, AND IN WHICH THE CORRECTIONS WERE GIVEN IN GRAMS.

Set Standard........	2	4	6	8	10	12	14	16
Subject A:								
Av. of standard pull........	1,393	1,235	1,485	1,725	1,425	1,224	1,163	967
A. D..........	*174*	*97*	*108*	*266*	*99*	*127*	*85*	*124*
Average error..	117	116	118	201	113	91	68	81
Variable error..	83	64	84	112	79	52	42	55
Ratio V. E. to stand........	*16.8*	*19.3*	*17.7*	*15.4*	*18.1*	*23.6*	*27.7*	*17.6*
Subject B:								
Av. of standard pull.........	516	430	462	527	562	432	492	601
A. D..........	*57*	*25*	*40*	*53*	*37*	*30*	*43*	*47*
Average error..	51	33	39	52	41	42	52	56
Variable error..	31	22	24	31	28	27	31	42
Ratio V. E. to stand........	*16.6*	*19.6*	*19.3*	*17.0*	*20.1*	*16.0*	*15.9*	*14.3*
Subject C:								
Av. of standard pull.........	918	945	1,105	827	891	916	1,012	782
A. D..........	*77*	*84*	*97*	*46*	*44*	*61*	*65*	*42*
Average error..	71	86	86	47	61	75	76	43
Variable error..	44	50	47	30	41	40	48	29
Ratio V. E. to stand........	*20.9*	*18.9*	*23.5*	*27.6*	*21.7*	*22.9*	*21.1*	*27.0*
Subject D:								
Av. of standard pull.........	441	447	442	438	439	413	433	455
A. D..........	*20*	*19*	*17*	*13*	*15*	*19*	*16*	*24*
Average error..	19	24	24	22	19	22	18	27
Variable error..	11	14	15	12	11	15	11	14
Ratio V. E. to stand........	*40.0*	*31.8*	*29.4*	*36.5*	*39.9*	*27.6*	*39.4*	*32.5*
Averages:								
Standard pull..	817	764	873	879	829	746	775	701
A. D..........	*82*	*56*	*65*	*94*	*49*	*59*	*52*	*59*
Average error..	64	65	67	80	58	57	53	52
Variable error..	42	37	42	46	40	33	33	35
Ratio.........	*23.6*	*22.4*	*22.5*	*24.1*	*25.0*	*22.5*	*26.0*	*22.8*

According to Woodworth's interpretation referred to above this would mean that force is not controlled by factors perfectly correlated nor operating by pure chance. Time seems to have some part to play (Table IV.), since the ratio of the errors to the standard time (that is, the time of the first pull) is about the same with all forces. It cannot, however, be the controlling factor, since the relative error is greater than either force or extent.

An examination of the average and variable error records (Table III.) shows that as the resistance to a movement is increased the ability to make two movements of the same extent is increased while on the other hand the ability to reproduce a standard pull (shown by the average deviation of the standard) is the same regardless of the resistance opposed to the movement, which means that the production of a standard force increases in direct proportion to the magnitude of the force (Weber's law). This together with evidence we will presently adduce points to the importance of extent in judging force.

Table V

Extent Records in Mm. of 1,600 Experiments in which the Subjects Attempted to Produce Two Movements of Equal Extent, and in which the Corrections Were Given in Mm.

Number of Springs	2	4	6	8	10	12	4	16
Subject A:								
Av. of standard pull	172.18	168.78	173.02	168.22	171.94	173.4	171.62	174.06
A. D.	*6.16*	*6.2*	*8.72*	*6.72*	*6.64*	*4.03*	*6.24*	*6.24*
Average error	10.0	7.1	9.4	10.4	6.9	5.0	5.26	6.7
Variable error	5.8	2.1	5.68	5.7	4.2	2.9	3.24	3.9
Ratio V. E. to stand.	*29.7*	*80.3*	*30.5*	*29.7*	*40.9*	*59.9*	*53*	*44.7*
Subject B:								
Av. of standard pull	182.58	177.3	175.4	179.2	178.72	172.86	172.72	171.78
A. D.	*13.12*	*11.86*	*7.34*	*7.02*	*7.21*	*8.88*	*5.56*	*5.36*
Average error	15.7	7.92	10.0	9.2	7.1	8.9	6.3	5.1
Variable error	7.1	4.4	5.6	5.0	3.3	2.3	4.4	2.7
Ratio V. E. to stand.	*25.7*	*40.3*	*31.3*	*35.8*	*54.1*	*75.0*	*39.2*	*63.6*
Subject C:								
Av. of standard pull	167.68	163.56	160.62	174.88	173.52	166.48	168.06	177.12
A. D.	*11.96*	*9.4*	*11.76*	*6.76*	*8.08*	*8.48*	*8.2*	*8.88*
Average error	13.04	12.62	10.52	5.8	9.14	9.28	9.7	9.34
Variable error	6.6	7.48	6.55	3.8	5.56	5.24	6.75	5.0
Ratio V. E. to stand.	*25.4*	*21.9*	*24.5*	*46.0*	*31.3*	*31.8*	*24.9*	*35.4*
Subject D:								
Av. of standard pull	174.24	175.46	181.9	173.88	178.04	174.68	175.02	169.44
A. D.	*7.9*	*8.32*	*7.32*	*6.98*	*8.04*	*8.1*	*5.1*	*7.56*
Average error	9.58	10.44	9.72	10.04	8.7	8.34	8.94	9.28
Variable error	5.56	5.85	5.03	6.55	5.28	5.12	5.36	6.32
Ratio V. E. to stand.	*31.4*	*30.0*	*36.1*	*26.5*	*33.8*	*34.1*	*32.7*	*26.9*
Averages:								
Standard pull	174.17	171.27	172.73	174.04	175.55	171.85	171.85	173.1
A. D.	*9.78*	*8.94*	*8.78*	*6.87*	*7.49*	*7.37*	*6.27*	*7.01*
Average error	12.08	9.52	9.91	8.86	7.96	7.88	7.55	7.61
Variable error	6.26	4.96	5.71	5.26	4.58	3.89	4.94	4.48
Ratio	*28.05*	*43.12*	*30.6*	*34.5*	*40.02*	*50.2*	*37.45*	*42.65*

With the same subjects another experiment was tried in which they were told to pull the handle a certain distance

and corrections made in terms of millimeters. A sitting was given with each group of springs used in the force standard experiments. Here an error of 100 grams with two springs

Table VI

Time Records in Sigma of 1600 Experiments in which the Subjects Attempted to Produce Two Movements of Equal Extent, and in which the Corrections Were Given in Mm.

Number of Springs..	2	4	6	8	10	12	14	16
Subject A:								
Av. of standard pull....	1,015	1,149	1,400	1,374	1,212	1,031	1,345	1,352
A. D.........	*68*	*106*	*171*	*147*	*92*	*48*	*79*	*100*
Average error.	60.2	87.4	138	125	85.6	52.5	99.8	96.8
Variable error.	39.1	25.8	92.5	65.4	44.2	37.2	51.7	58.3
Ratio V. E. to stand.......	*26.0*	*44.5*	*15.2*	*21.0*	*27.4*	*27.7*	*26.1*	*23.2*
Subject B:								
Av. of standard pull....	503	488	432	476	616	513	497	550
A. D.........	*39*	*37*	*24.5*	*41.2*	*21.6*	*28.2*	*26.5*	*33.1*
Average error.	44.8	38	33.6	33.4	40.3	40.3	28.6	26.6
Variable error.	26.7	20.3	23.3	20.3	25.5	23.8	16.7	20.5
Ratio V. E. to stand.......	*18.9*	*24*	*18.6*	*23.4*	*24.1*	*21.6*	*29.8*	*26.8*
Subject C:								
Av. of standard pull....	955	942	545	1,155	1,045	924	762	1,035
A. D.........	*84.4*	*70*	*84.7*	*103.5*	*70.9*	*71.7*	*58.6*	*66.7*
Average error.	70.1	64.2	53.3	37.8	61.7	72.3	57.3	89.0
Variable error.	48	37.3	31	56.8	37.3	41.8	38.5	51.7
Ratio V. E. to stand.......	*19.9*	*25.3*	*17.6*	*20.4*	*28*	*22.1*	*19.8*	*20*
Subject D:								
Av. of standard pull....	479	428	512	441	418	504	470	448
A. D.........	*25.4*	*27.9*	*34.1*	*20.5*	*19.6*	*27.9*	*47.2*	*16.8*
Average error.	31.4	30.7	34.1	32.5	30.0	25.0	45.7	29.7
Variable error.	16.4	20.6	22.9	19.4	19.7	19.4	30	18.9
Ratio V. E. to stand.......	*29.2*	*20.8*	*22.4*	*22.7*	*21.2*	*26.0*	*15.7*	*23.7*
Averages:								
Standard pull.	738	752	722	861	823	743	768	846
A. D.........	*54.2*	*60.2*	*78.6*	*78*	*51*	*44*	*52.8*	*54*
Average error.	51.6	55	64.7	57.2	54.4	47.5	57.8	60.5
Variable error.	32.5	26	42.4	40.5	31.7	30.5	34.2	37.3
Ratio........	*23.5*	*28.6*	*18.4*	*21.9*	*25.1*	*24.3*	*22.8*	*23.4*

would involve the same extent error as an error of 800 grams with 16 springs. The records of this experiment are however not materially different from the records when a force standard was used (Tables V. and VI.). When identical conditions exist efforts to reproduce a standard force or a standard

extent produce identical results (see Fig. 3). When stated in terms of force we have found that as the magnitude of the stimulus increases the variable error increases more slowly than in direct proportion to the stimulus, but more rapidly than in proportion to the square root of the stimulus. When

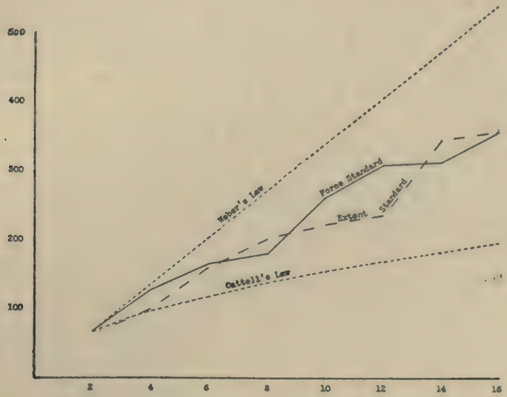

FIG. 3. This graph shows the force records of subjects *A*, *B*, *C* and *D*, when they were guided by both a force and an extent standard, compared with the records that would be expected from Weber's and Cattell's psycho-physical laws.

stated in terms of extent we have found that as the resistance offered to a movement of the same extent increases the accuracy with which the movement can be reproduced increases. We cannot however make too much of this point because, as we shall show later, our other four subjects do not show this reaction. We may suggest as an explanation that our later four subjects may have used extent as a basis of judgment to a greater extent than did our first four.

With the end in view of examining further this relation between force and extent, it was planned to eliminate extent altogether and to see what effect this would have upon the subject's judgment and control of force. The dynamometer was suspended so that it would swing freely, the subject grasped a handle fastened to the rear of the dynamometer, and the experimenter increased or decreased the tension by means of a windlass attached by a cord to the handle. In this experiment, as before, twenty-five practice trials were

given, after which the pulls were made in pairs. The spring was tightened until the subject gave a vocal signal that what he deemed was the standard had been reached. All the tension was then removed and again the spring tightened until the subject gave the signal. The spring was tightened with approximately the same speed that the subjects used in making the movement themselves. Of course with such a procedure a greater variability would appear in the results due to the introduction of the reaction time of the experimenter. However, this increase in the error records would be the same regardless of the amount of the force standard and so would not prevent a study of the relations of the different force standards.

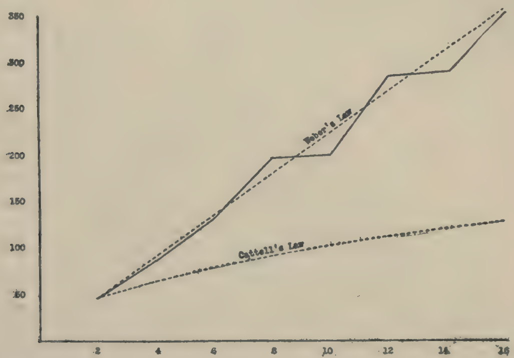

Fig. 4. This graph shows the force records of subjects *E*, *F*, *G* and *H* when they were guided by a force standard in pulling the dynamometer compared with the records that would be expected from Weber's and Cattell's laws.

As new subjects (*E*, *F*, *G* and *H*) were used in this experiment, additional experiments were made with them using the same procedure as with the subjects *A*, *B*, *C* and *D*, with the exception that no time records were taken. Contrary to the former results, the variable errors (Table VII.) followed very closely Weber's law (Fig. 4). This means that these subjects made errors of approximately the same extent when pulling with a standard force of 16 kilograms as with a force standard of 2 kilograms, indicating that they were largely influenced by extent. The records where extent was

Table VII

Force Records in Grams of 1600 Experiments in which the Subjects attempted to Produce Two Movements of Equal Force, and in which Corrections Were Given Them in Grams.

Set Standard.....	2	4	6	8	10	12	14	16
Subject E:								
Av. of standard pull....	1,981	4,027	5,910	7,634	9.718	12,520	13,427	15,819
A. D.........	*46*	*122*	*199*	*351*	*470*	*478*	*636*	*598*
Average error	49	142	263	546	649	439	593	544
Variable error	28	72	150	290	298	249	291	326
Ratio V. E. to stand......	*70.7*	*29*	*39.4*	*26.3*	*32.6*	*50.3*	*46.2*	*48.5*
Subject F:								
Av. of standard pull....	2,033	3,992	6,134	8,162	9,900	12,275	13,744	16,495
A. D.........	*135*	*144*	*297*	*349*	*382*	*508*	*683*	*605*
Average error	85	109	180	246	296	374	424	366
Variable error	51	73	80	104	109	170	272	221
Ratio V. E. to stand......	*39.8*	*54.6*	*76.6*	*78.5*	*90.8*	*72.2*	*50.6*	*74.6*
Subject G:								
Av. of standard pull....	2,095	3,972	5,771	7,700	9,643	11,506	13,581	15,086
A. D.........	*107*	*160*	*288*	*372*	*362*	*577*	*518*	*828*
Average error	108	236	431	510	467	1,120	794	1,374
Variable error	70	133	204	254	263	552	369	544
Ratio V. E. to stand......	*42.2*	*29.7*	*28*	*30.3*	*40.8*	*20.9*	*36.8*	*27.7*
Subject H:								
Av. of standard pull....	1,999	4,090	5,969	7,992	10,275	12,149	14,270	16,210
A. D.........	*80*	*93*	*175*	*262*	*354*	*316*	*515*	*528*
Average error	71	107	159	259	286	253	335	466
Variable error	34	73	100	141	163	168	231	330
Ratio V. E. to stand......	*58.8*	*56.1*	*56.7*	*56.7*	*63.4*	*72.3*	*61.8*	*49.2*
Averages:								
Standard pull	2,027	4,020	5,946	7,872	9,884	12,112	13,755	15,902
A. D.........	*92*	*130*	*240*	*333*	*392*	*470*	*588*	*640*
Average error	78	148	258	390	424	546	536	687
Variable error	46	88	133	197	201	285	291	355
Ratio........	*52.9*	*42.3*	*50.2*	*48.0*	*56.9*	*53.9*	*48.8*	*50.0*

eliminated do not follow Weber's law but fall between Weber's law and Cattell's law (Table VIII. and Fig. 5). This result is not merely a result of the difference in method, but a result of the elimination of extent. Evidence for this is given by a study of the judgment thresholds. As we noted above after each pair of pulls the subject gave his judgment as to whether the second was heavier or lighter than the first. From these judgments thresholds were calculated and are

Table VIII

Extent Records in Mm. of 1,600 Experiments in which the Subjects Attempted to Produce Two Movements of Equal Force, and in which Corrections Were Given in Grams.

Set Standard	2	4	6	8	10	12	14	16
Subject E:								
Av. of standard pull	172.1	174.34	169.04	162.86	166.36	184.66	166.82	173.74
A. D.	*4.64*	*6.12*	*6.64*	*8.76*	*9.4*	*7.96*	*9.08*	*7.48*
Average error	4.92	7.1	8.78	13.66	12.98	7.32	8.46	6.8
Variable error	2.8	3.6	5.0	7.24	5.96	4.16	4.16	4.08
Ratio V. E. to stand.	*61.5*	*48.5*	*33.8*	*22.5*	*27.9*	*44.4*	*40.2*	*42.6*
Subject F:								
Av. of standard pull	177.28	172.56	176.46	176.04	170.00	180.58	171.34	182.18
A. D.	*13.52*	*7.2*	*9.92*	*8.72*	*7.64*	*8.64*	*9.76*	*7.56*
Average error	8.52	5.44	5.98	6.14	5.92	6.24	6.06	4.58
Variable error	5.14	3.64	2.68	2.6	2.18	2.84	3.88	2.76
Ratio V. E. to stand.	*34.5*	*47.4*	*65.5*	*67.6*	*78.0*	*63.6*	*44.2*	*66.0*
Subject G:								
Av. of standard pull	183.54	171.62	164.38	164.5	164.86	167.76	169.02	164.58
A. D.	*10.72*	*8.02*	*9.6*	*9.32*	*7.24*	*9.64*	*7.4*	*10.36*
Average error	10.84	11.8	14.34	12.76	9.36	18.66	11.36	17.18
Variable error	7.04	6.68	6.8	6.4	4.72	9.2	5.28	6.92
Ratio V. E. to stand.	*26.1*	*25.7*	*24.2*	*25.7*	*34.8*	*18.2*	*32.0*	*23.8*
Subject H:								
Av. of standard pull	173.9	177.52	170.98	171.8	177.7	178.48	178.86	178.62
A. D.	*8.02*	*4.64*	*5.84*	*6.56*	*7.08*	*5.28*	*7.36*	*6.6*
Average error	7.06	5.36	5.3	6.34	5.72	4.22	4.78	5.84
Variable error	3.4	3.64	3.32	3.52	3.26	2.8	3.3	4.13
Ratio V. E. to stand.	*51.1*	*48.8*	*51.8*	*48.8*	*54.5*	*63.7*	*54.2*	*43.2*
Averages:								
Standard pull	176.6	174.01	170.09	168.8	169.73	177.87	171.51	174.78
A. D.	*9.22*	*6.49*	*8.0*	*8.34*	*7.84*	*7.88*	*8.4*	*8.0*
Average error	7.83	7.42	8.6	9.72	8.5	9.11	7.66	8.6
Variable error	4.6	4.39	4.45	4.94	4.03	4.75	4.15	4.47
Ratio	*43.3*	*42.6*	*43.82*	*41.15*	*48.8*	*47.48*	*42.65*	*43.9*

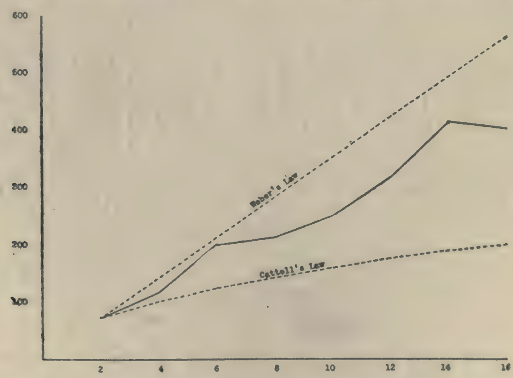

Fig. 5. This graph shows the force records of subjects *E*, *F*, *G* and *H* when they held the arm stationary in judging the tension of the spring compared with the records that would be expected from Weber's and Cattell's laws.

given in Table IX. In the experiments where the subjects made arm movements in pulling the dynamometer the threshold follows Weber's law (Fig. 6); where the subject held his arm stationary it falls between Weber's law and

TABLE IX

FORCE RECORDS IN GRAMS OF 1,600 EXPERIMENTS IN WHICH THE SUBJECTS ATTEMPTED TO JUDGE THE POINTS IN TWO SUCCESSIVE TRIALS WHEN THE TENSION ON A HANDLE, WHICH WAS HELD STATIONARY, WAS EQUAL.

Set Standard.....	2	4	6	8	10	12	14	16
Subject E:								
Av. of standard tension.	1,992	3,986	6,006	7,623	9,973	11,069	13,846	16,470
A. D.........	*151*	*164*	*298*	*302*	*318*	*482*	*358*	*396*
Average error	157	188	350	543	337	605	690	400
Variable error	80	110	228	254	212	341	448	202
Ratio V. E. to stand......	*24.9*	*36.2*	*26.3*	*30.0*	*47.0*	*32.4*	*31.0*	*81.5*
Subject F:								
Av. of standard tension.	2,141	4,413	6,464	8,422	10,224	12,814	14,384	16,357
A. D.........	*140*	*234*	*257*	*422*	*452*	*396*	*617*	*697*
Average error	109	201	361	343	581	433	686	694
Variable error	56	105	190	192	286	199	350	358
Ratio V. E. to stand......	*38.2*	*42.0*	*34.0*	*43.8*	*35.8*	*64.4*	*41.0*	*45.7*
Subject G:								
Av. of standard tension.	2,184	4,000	5,996	8,280	10,542	12,245	14,475	16,906
A. D.........	*96*	*182*	*280*	*363*	*338*	*451*	*484*	*739*
Average error	172	190	401	354	498	458	663	1,133
Variable error	102	123	208	181	306	214	624	560
Ratio V. E. to stand......	*21.4*	*32.5*	*28.8*	*45.7*	*32.8*	*57.2*	*23.2*	*30.2*
Subject H:								
Av. of standard tension.	2,125	4,133	6,278	8,268	10,451	12,632	14,437	16,741
A. D.........	*113*	*185*	*256*	*321*	*324*	*482*	*392*	*595*
Average error	109	199	416	487	309	812	434	940
Variable error	54	131	224	218	202	518	235	509
Ratio V. E. to stand......	*39.4*	*31.5*	*28.0*	*37.9*	*51.8*	*24.4*	*61.4*	*33.0*
Averages:								
Standard tension.......	2,111	4,133	6,186	8,148	10,297	12,190	14,285	16,618
A. D.........	*125*	*191*	*273*	*352*	*358*	*453*	*463*	*607*
Average error	137	194	382	432	431	577	618	792
Variable error	73	117	202	211	251	318	414	407
Ratio........	*31.0*	*35.5*	*29.3*	*39.3*	*41.8*	*44.6*	*39.1*	*47.6*

Cattell's law (Fig. 7). Referring again to Woodworth's interpretation this means that when the subject makes the arm movement the judgment of force depends on a factor or

factors closely correlated with force. When extent is ruled out the judgment of force is determined by factors related more by chance. This indicates that with some subjects at least the judgment of force is closely correlated with judgment of extent.

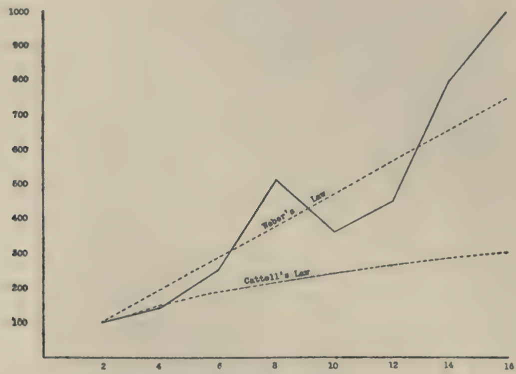

Fig. 6. This graph shows the judgment thresholds of subjects *E*, *F*, *G* and *H* when they had a force standard in pulling the dynamometer compared with what would be expected from Weber's and Cattell's laws.

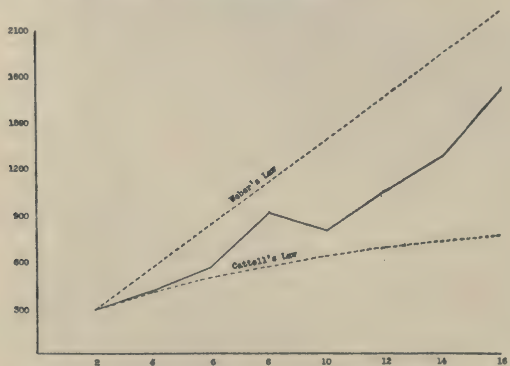

Fig. 7. This graph shows the judgment thresholds of subjects *E*, *F*, *G* and *H* when they held their arm stationary while the experimenter changed the tension of the dynamometer spring compared with what would be expected from Weber's and Cattell's laws.

In Table X. we have assembled the judgment thresholds derived from the different experiments. In part *A* are given the judgment thresholds when the subjects attempted to

Table X

A. Extent Thresholds Based on the Judgments of the Subjects in 1,600 Experiments in which the Attempt Was Made to Produce Two Movements of Equal Force. Unit 1 mm.

Set Standard..	2	4	6	8	10	12	14	16
Subjects								
A.......	9	11	8	14	10	11	8	8
B.......	11	13	13	12	16	9	10	16
C.......	7	7	11	9	16	24	14	19
D.......	24	22	30	6	8	19	27	17
Average....	12.75	13.25	13	10.25	12.5	15.75	14.75	15

B. Extent Thresholds Based on the Judgments of the Subjects in 1,600 Experiments in which the Attempt Was Made to Produce Two Movements of Equal Extent. Unit 1 mm.

Set Standard..	2	4	6	8	10	12	14	16
Subjects								
A.......	13	11	11	11	13	7	9	12
B.......	18	10	12	13	15	7	7	8
C.......	13	14	15	8	7	12	17	10
D.......	13	17	12	11	10	10	14	15
Average....	14.25	13	12.5	10.75	11.25	9	11.75	11.25

C. Force Thresholds Based on the Judgments of the Subjects in 1,600 Experiments in which the Attempt Was Made to Produce Two Movements of Equal Force. Unit 1 gram.

Set Standard..	2	4	6	8	10	12	14	16
Subjects								
E.......	50	100	270	840	500	420	630	1,040
F.......	180	100	210	320	300	720	910	1,040
G.......	90	200	330	240	300	360	560	880
H.......	110	180	300	680	350	300	1,120	1,120
Average....	107.5	145	277.5	520	362.5	450	805	1,020

D. Force Thresholds Based on the Judgments of the Subjects in 1,600 Experiments in which the Attempt Was Made to Indicate the Points in Two Successive Trials When the Tension on a Handle, which Was Held Stationary, Was Equal. Unit 1 gram.

Set Standard..	2	4	6	8	10	12	14	16
Subjects								
E.......	250	560	900	1,240	500	1,440	2,170	1,520
F.......	210	280	270	680	750	780	980	1,520
G.......	520	340	480	720	1,200	840	1,050	1,520
H.......	140	420	570	1,040	500	1,200	980	2,240
Average....	280	400	555	920	737.5	1,065	1,295	1,700

pull with a given force and were corrected in terms of grams; in part *B* are the records from the experiments where the subjects attempted to pull a certain extent and were given corrections in millimeters. When they had the force standard the errors in judgment increased as the size of the standard increased. When they had the extent standard the errors in judgment became smaller as the resistance to be overcome increased. This is interesting when we consider that in each of these cases the subject had the same sensation complex. If he was told to pull 16 kilograms and had to make a movement 175 millimeters long to do so, or if he was told to pull the handle 175 millimeters and had to pull 16 kilograms to do this, the only factor changed was the difference in instructions. In some way the difference in the two standards striven for must have changed his interpretation of the same sensation complex. As we have shown above the variable errors in the two experiments were not materially different (Tables II., III., V. and Fig. 3). There must therefore have been something in the experiment that rendered their judgments different in the two cases while it did not change their accuracy of movement. In all probability this was the nature of the corrections given. When a subject made an extent error of 1 millimeter and was told that he had made an error of 80 grams in one case (with a 16 kilogram standard), and 10 grams (2 kilogram standard) in another, the effect would be different from that induced if with two similar pulls with different forces he was told that in each case he had made an error of 1 millimeter. It must be remembered that in no case was the subject told whether he had made a correct or false judgment, but after he had made two pulls he was told how the latter pull differed from the arbitrary standard of the experiment. When a subject had made two pulls which he thought were both near the standard and was told that in one case the latter pull was 80 grams heavy and in the other 1 millimeter long he would in the first instance be led to make a larger correction, or at least have his confidence in his accuracy shaken, while in the latter case he would think he had done well and attempt to do the same the next time.

Part *D* of Table X. is a tabulation of the judgment thresholds when the subject merely held his arm stationary and the tension on the handle was changed by the experimenter. Part *C* is a tabulation of the judgments of these same subjects when they made arm movements. We have said above that the reaction time of the experimenter in the former situation probably made the variable errors larger, but there is no reason why this should have made the judgment thresholds differ. Yet we find that when the extent factor is eliminated the thresholds are from 1.6 to 2.75 (average 2.1) times as large as when the extent element was present. It seems unquestionable that when a subject could he used sensations of extent to help him judge the force of a movement just made by him, when extent was eliminated he could judge with less accuracy the force of his movements although he could judge them with some degree of accuracy.

We have seen that whether an extent or force standard were used the subjects kept their time fairly uniform. While the ratio of the variable error in time to the total time of the pull shows that taken by itself it is more variable than either extent or force, it may nevertheless play a large part in the control and judgment of the other two factors. We therefore tried a short supplementary experiment to ascertain the effect of radical variations in time.

The procedure last described was used, namely that of having the subject hold the handle while the experimenter changed the tension of the springs; and, as these subjects had no previous knowledge of the experiments, they were given a series of trials with the arm movement procedure for comparison. The standard used was 10,000 grams. When the subjects made arm movements the judgment thresholds of the two subjects were (*I*) 650 grams and (*J*) 350 grams. When they held the handle in a fixed position and the speed with which the springs were tightened was radically different in the two trials of each pair according to a prearranged schedule, the thresholds were (*I*) 1,050 and (*J*) 1450. When the speed of release was changed as well as the speed of tightening, that is, when the spring was

tightened quickly, care was taken to release it quickly and vice versa; the thresholds were respectively 1,250 and 2,200 grams. Here where extent was eliminated and time so varied as to eliminate it as a help the judgment threshold showed a ratio of 1 to 8 and 1 to 4.5. Where extent was eliminated and no attempt was made to change radically the time the judgment thresholds averaged 1 to 10.8, and when the subject made the movement and so could have the benefit of extent and time the average judgment ratio was 1 to 21.3.

V. Conclusions

An outstanding feature of these experiments is the adaptation of the subjects to changed conditions. Adaptation in fairly complex situations is generally recognized under the name of learning and an individual's intelligence is judged by the extent to which and the speed with which he makes a selection of the most favorable reaction toward any given situation. If he makes the most intelligent reaction he will respond in such a way as to produce a certain result in the most efficient way. If after he has learned this most expeditious form of reaction some block is put in his way or this mode of action prevented in any way, he does not then and there give up all effort to attain the end in view but tries some new method of attack. All this is perfectly obvious and well recognized. That the same thing holds in more elementary fields has likewise been recognized by biologists and psychologists, but it has not received the emphasis which it deserves.

It is clear from our experiments that to execute a movement of a certain force is a learned act and that to make a judgment of the force of a movement that has been made is also a learned act. Like every learned act it requires practice for one to become at all skilled in it and like any complex process it is easily interfered with by a change in the circumstances surrounding the act. The subject when asked to pull a handle 175 millimeters, when asked to pull with a force of 8 kilograms, or when asked to hold his arm stationary and signal when the tension has reached 8 kilograms, must

in the first instance make a random reaction just as one does in learning a strange puzzle or a maze. This random reaction produces a certain complex of sensations, and when he is told that he has made an error he tries to change his next movement so as to make the correction and is guided in this attempted correction by a comparison of the second sensation complex with the memory of the first. There may be various elements that he uses in successive trials, just as one makes various efforts to solve any new situation, and the final result of the elimination process is to select that element which will give him the best results.

It is important that the complex nature of the perception of force be clearly recognized. Investigators have spoken of it as though it were some simple sensation and statements have been made that weights were judged not by force but by the distance to which they were raised, by the speed of the movement, or by the latent time between the initiation of effort and the actual movement of the weight. Others have contended that weights are judged by sensations of force. The parties on both sides of the argument seemed to consider force as an elementary process, and through failure to analyze it as a complex process, could not agree.

We have shown that one is best able to learn to produce a movement of a certain force when extent and time are both involved. The elimination of extent greatly interferes with the act, as does any radical variation in the time. When these modifications are introduced, however, one can learn to produce movements approximating the desired force, but with less efficiency than when the extent and time factors are present.

We believe that the evidence herein adduced is sufficient to prove that the force of a movement is controlled by a number of factors; that extent is for most individuals a dominant factor and, though with less certainty, that time is an important factor. Besides these two there are a number of less closely correlated factors that an individual uses when he is prevented from using extent and time. That there is a simple sensation of force seems out of the question.

THE TONE INTENSITY REACTION

BY A. P. WEISS

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The purpose of this experiment was to investigate the sound intensity reaction when tones of the same frequency (256 double vibrations per second) but of different intensities were compared. The aim was not so much to determine the limen for tone intensities as it was to furnish the facts upon which might be built a provisional theory of the neuro-muscular basis of the sound intensity reaction. The experiment was undertaken from the standpoint of behaviorism and the requirements differ somewhat from those of the ordinary limen experiment as usually understood in psychology.

Part I. treats of an experimental study in tone intensity discrimination and Part II. considers the results of Part I. from the theoretical standpoint in an attempt to determine the character of the neuro-muscular basis of the sound intensity reaction.

I. EXPERIMENT ON TONE INTENSITY DISCRIMINATION

Apparatus

The experiment required a pure tone which could be easily and accurately varied in its intensity.

Fig. 1 is a diagram of the parts of the apparatus. The tones which were compared were produced by two electrically driven forks.

Driving Fork.—The driving fork 5 is an ordinary electrically driven fork. The direct current from the terminals D.C. passes through the magnet 3 then into the fork base to be interrupted by the vibrations of the prongs at the contact 4. A rheostat at 8 makes it possible to control the amount of current which passes through the magnet 3 and in

this way the amplitude of the fork is maintained at optimal make-and-break conditions.

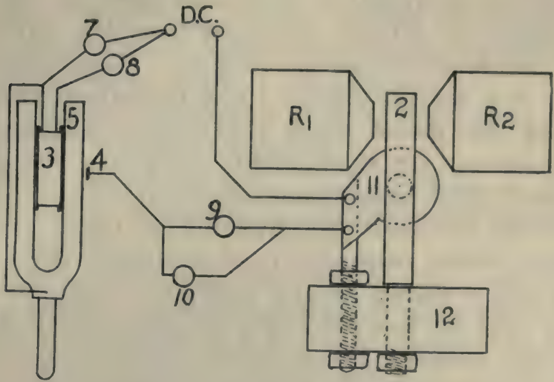


FIG. 1. Diagram of connections.

Tone Fork.—The current which is interrupted at 4 also passes through the magnet 11 of the tone producing fork 2. This fork is located in the experimental room. There are no contacts on this fork, but since its period is the same as that of the driving fork 5 its vibrations are maintained by the current which is being interrupted at 4. A rheostat at 9 controls the amount of current passing through the magnet 11. The rheostat at 7 makes it possible to pass a greater amount of current through the magnet 11 of the tone fork than is passing through the magnet 3 of the driving fork. The amplitude of both forks may thus be controlled independently. At 10 is placed a voltmeter which reads accurately to one tenth volt. This is to check the fluctuations in the current strength and also to keep the current through the tone fork magnet constant. Since there are no contacts on this fork, its amplitude can be controlled quite accurately by changing the magnitude of the current.

The tone fork is kept vibrating at a constant amplitude all the time and to make it inaudible it is mounted on a heavy cast iron block 12 and suspended from a frame by ropes. In this way the prongs can not act on any considerable volume of air and the fork vibrates silently except as will be presently indicated.

Resonators.—The tones which are compared are produced by the action of the tone fork 2 upon the resonators $R1$ and $R2$ which are mounted on guides so they can be moved independently to different distances from the fork prongs. Both of the resonators are made of tin. The back of each resonator is a wooden piston. The resonators were tuned by shifting this piston where the resonance seemed to be at the optimum, and then sealed in place by hot paraffin. The magnet which opened and closed the shutter over the mouth of the resonator was mounted on this wood piston. The resonators are suspended on a rod above the tone fork in the position shown in the drawing. They are moved back and forth on this rod and a convenient scale indicates the distance between the resonator and the fork prongs. When the shutter over the mouth of the resonator is down, the resonator is silent except when less than 20 millimeters from the fork prongs. At this distance the tone is heard very weakly. This weak tone is due to the air impulses from the fork being transmitted through the closed shutters, causing the resonators to sound to a weak degree. This does not act as a disturbing factor because this residual tone occurs only when the intensities to be compared are the loudest in the series.

Timing Device.—The opening and closing of the resonator shutters are produced by a timing device[1] so that the tones are always presented in a given order and duration. The tones alternate so that while one resonator is being opened the other is being closed. The electrical connections are of such type that overlapping of the tones cannot take place. This also avoids the interference effect which manifests itself when two resonators are placed with their openings opposite each other. The timing device is controlled by a second's pendulum acting on a toothed wheel which makes electrical contact in such a manner that for each observation or judgment four tones are presented. Each resonator is opened twice and each tone lasts one second. The resonator tones are acoustically pure. Occasionally a nut or a washer gets

[1] 'Pendulum and Interval Timer,' Psychol. Rev., 1916, **23**, 508–516.

loose and begins to rattle or hum, but such disturbances are temporary and easily corrected. Toward the end of the experiment some of the observers noticed a difference in the quality of the two resonators. To the writer, this seemed to be an illusion of a difference in pitch, but it did not seem to interfere with the judgment of *intensity*.

There were no starting and stopping difficulties; the tones 'came in' and 'went out' smoothly.

Operation and Calibration.—The intensity of the tones was controlled by shifting the resonators to different distances from the tone fork.

The resonators did not have exactly the same resonance qualities, but he who has ever tried to make two resonators alike will understand that it is practically impossible to do this. It is simpler to make them as nearly alike as possible and then equate them for intensities by a subjective method. This was done by drawing a curve for the resonator used as a standard and plotting against these values those of the comparison resonator.

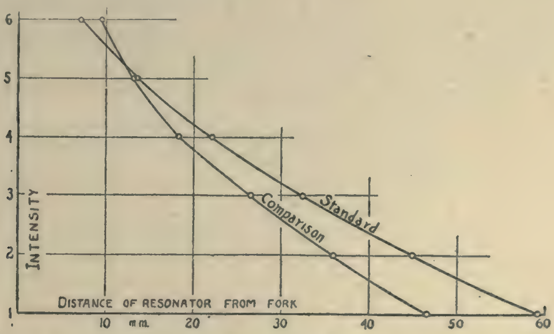


FIG. 2. Curve showing distances from the fork in millimeters at which the standard and comparison resonators were subjectively equal in intensity.

Fig. 2 shows the curves for the standard resonator and the corresponding position at which the comparison resonator was judged equal in intensity to the standard resonator. The values for the comparison resonator are the averages of 13 observers. The most obvious difference between the two resonators shows that the comparison resonator speaks louder

than the standard when near the fork, but weaker than the standard when farther away. Both speak with the same intensity when 13 mm. from the fork as is shown by the point at which the curves intersect.

The apparatus as a whole was practically automatic. The resonator shifts were quickly made and the four tones were always presented in the same way. The experimenter merely pressed the starting key for the timing device long enough to start the series, which then automatically ran to a finish and set itself ready for the next series.

The amplitude of the tone fork was kept at about 1 millimeter. In a preliminary set of experiments it was found that the amount of current passing through the magnet of the tone fork was a good index of the amplitude and that a greater constancy of amplitude could be secured by regulating the resistance in the circuit than by trying to make microscopic readings of the amplitude from time to time. For accurate determinations of the limen for tone intensities this method will not be satisfactory, but until a serviceable standard for tone intensities has been developed the degree of uniformity secured by keeping the current through the magnet 11 at a constant difference in potential will meet all practical requirements. At no time could any change in the loudness of the tones be detected. There was however no assurance that the *absolute* intensities of the tones remained exactly the same during the experiment.

Measuring the absolute intensities of tones is one of the difficult problems in physical measurements. The energy values are very small and the relation between objective and subjective intensities has not been clearly established. For the purpose of this experiment the *ratios* of the various intensities were of greater significance than the absolute values and these ratios were practically constant because they were derived from the same source of sound. Any variation in the absolute intensity of the tone fork 2 would thus affect both the standard and comparison intensity in equal degrees.

Experimental Method

Range of Intensities.—Six intensities (all of the frequency 256) were used as standards in this experiment. As there is no absolute standard, only a general description of the intensities can be given. At six feet from the resonator the weakest intensity (1) could easily be heard. The tone was weak but clear enough so that judgments were confidently made. The strongest intensity (6) was not very loud—about of the degree of loudness which can be secured by a skilled operator in bowing a good 256 fork on its resonance box. The six intensities used as standards were the intensities when the standard resonator was 7.3, 13.7, 22.1, 32.5, 44.9, 59.3 millimeters from the fork prongs vibrating with an amplitude of 1 mm.

When the resonators are near to the fork the just noticeable differences in intensities represent much shorter steps than when the resonators are farther away. The law according to which these j.n.d.'s increase in length is approximately a logarithmic series. In order to have all the steps of the intensity scale of about equal difference so far as simple objective measure will permit this, it was decided to form a logarithmic series with the base of 0.1 by squaring the arithmetic series .1, .2, .3, .4, etc., to 7.7, which was the upper limit. The lower limit was taken as the 27th multiple of .1 which when squared equals 7.3 mm. When the resonator is moved too near the fork prongs its tone becomes irregular and weaker, and for this reason the first step was taken at 7.3 rather than close to the fork. The square of the upper limit 7.7 is 59.3 mm. Beyond this it was found that the tones became too weak. The 55 steps between the lower and upper limits were divided into the six equal parts which were used as the six standards as indicated opposite 'distance' in Table II. The difference between adjacent intensities was ten steps.

Method of Making Judgments.—The method used in making the judgments or reactions was a combination of the Method of Paired Comparisons and Right-and-Wrong Cases. The tones which were to be compared with respect to intensity

were presented in two pairs. The observer was required to judge whether the last tone was 'stronger' or 'weaker' than the second last tone. The first and second intensities were the same as the third and fourth respectively, and were given merely as a sort of ready signal and to prepare the observer for the last two tones. In this way the observer was able to make a provisional judgment during the first two tones and the last two tones were then used to corroborate this provisional judgment.

The standard resonator was always kept at one of the six distances selected as standard while the comparison resonator was shifted to various positions at random. Judgments of 'equal' or 'doubtful' were not permitted. In 'doubtful' cases the tones were repeated until the observer was able to make a one-direction judgment. In about a dozen instances the observer insisted on making equal judgments, which were recorded as both weak and strong. It was found that if 'doubtful' or 'equal' judgments are accepted, the observer will not make his observations so carefully. Extreme differences in intensity were avoided. Most of the observations were restricted to the region in which the observer made 'mixed' judgments. This saved time and also kept the attention at a maximum.

Observers.—Of the observers used in this experiment *W* was the writer and *G* his wife. Both have had a great deal of training in making sound intensity judgments. *Cu* and *Go* are instructors in the department of psychology and while not specifically trained in tone intensity discrimination they have had considerable practice in psychological experiments. The other observers were advanced students in psychology who had taken or were taking a laboratory course in experimental psychology. The writer takes this occasion to thank his observers for their courtesy and the sacrifice of their time. Both the writer and his wife conducted the experiment.

In column *A* are recorded the different distances in millimeters at which the comparison resonator was placed. Column *B* gives the number of judgments for each of the

positions in *A*. In *C* are given the number of times the judgment was 'strong' and in *D* the number of times it was 'weak.' In *E* is given the ratio of the 'strong' over the total number of judgments. When all the judgments are 'strong' the ratio is 1.00; when they are all 'weak' the ratio is zero; when half are 'strong' and half 'weak' the ratio is .50. Under *F* are recorded the number of times the observer asked to have the tones repeated. Each observer reported for all six intensities.

TABLE I

SPECIMEN DATA SHEET
Obs. *Wa.* Date Nov. 15, 1916
Standard Intensity 13.7 mm. (Intensity 5)

Comp. Intens. *A*	Judgments				
	Total *B*	Strong *C*	Weak *D*	Ratio *E*	Rep. *F*
8.4	0	0	0		
9.0	1	1	0	1.00	0
9.6	1	1	0	1.00	0
10.2	2	2	0	1.00	0
10.9	3	3	0	1.00	0
11.6	3	3	0	1.00	0
12.2	5	5	0	1.00	0
13.0	18	14	4	.78	1
13.7	23	17	6	.74	5
14.4	25	6	19	.24	5
15.2	22	7	15	.32	5
16.0	16	3	13	.19	0
16.8	5	0	5	.00	0
17.6	3	0	3	.00	0
18.5	2	0	2	.00	0
19.4	1	0	0	.00	0
21.2	0	0	0		

Critical range = 5 steps

Critical Range.—The number of steps under *A* which are included between the ratios of .90 and .10 are called the *critical range*. This region is one in which the observer's judgments are variable. For observer *Wa* in the specimen data sheet this critical range is the 5 steps, 13.0, 13.7, 14.4, 15.2, 16.0. The combinations of intensities are given in purely random order so that the observer can not anticipate the intensity relations. In order to keep the attention at a maximum most of the comparisons (about 75 per cent.) were given within the critical range.

Results

The critical range for the intensities ranged from 4 to 9 steps as indicated in the last line, with an average for the six intensities of 6 steps. Since the difference between adjacent standards was ten steps the differences between the standards were supraliminal, and since the whole range of intensities included 55 steps there were about 9 j.n.d.'s on a basis of 80 per cent. correct judgments. The critical range of intensity 3 (4 steps) is less than half that of intensity 5 (9 steps) and the lack of uniformity among the other intensities is greater than was anticipated. It seems more probable that this is due to apparatus difficulties which passed undetected, rather than to actual anatomical irregularities in the ear.

Table II

Critical Range of the Various Observers and for the Six Intensities

Intensities Distance (Std.)	1 59.3	2 44.9	3 32.5	4 22.1	5 13.7	6 7.3	Av.	Av. Dev.
Observers								
G	3	5	4	7	13	7	6	2.5
W	3	5	5	5	16	4	6	3.0
Wi	4	5	8	7	14	8	8	2.3
Cu	4	5	2	6	8	2	4	1.8
Ho	11	13	11	5	8	8	9	2.3
Sp	6	7	5	1	5	2	4	2.0
Wa	10	10	8	8	8	18	10	2.3
Eb	2	3	3	3	11	8	5	3.0
Es	8	13	2	8	8	4	7	2.8
Da	1	6	3	2	4	3	3	1.2
Ro	8	5	1	2	1	6	4	2.5
Go	2	3	2	4	2	2	3	0.8
Sa	9	11	4	7	19	6	9	3.7
Av. for inten.	5	7	4	5	9	6	6	

The individual differences in the critical ranges, as shown in the second-last column, vary from three to ten steps. This means that the acuity for the discrimination of tone intensities may be three times greater in one individual than in others. This difference is considerably less than the individual differences in pitch discrimination.[1]

The variability of the observers is indicated by the last

[1] Stumpf, C., 'Tonpsychologie,' I., 1883, section 14.

column, which shows the average deviation in steps. The range .8 to 3.7 or in the ratio of about 1 to 5 indicates the extent of the individual variation.

Constant Displacement of 'Equal' Point.—During a series of observations the experimenter frequently noticed that the observer was making judgments which to the experimenter seemed to be wrong. The observer would call a tone 'weaker' which to the observer seemed 'stronger.' The reverse condition also occurred. The observers were perfectly consistent within their own limits. Thus *Cu* in the average of all his observations judged the comparison tone five steps stronger than the standard. That is, at the point at which the other observers judged the two intensities equal *Cu* would not report them equal until the comparison tone was made five steps stronger. *Wi*, on the other hand, did not judge them equal until the comparison tone had been made three steps weaker.

These anomalies were noticed by both experimenters. Until this fact can be made the basis of a special investigation we shall content ourselves with merely mentioning the phenomenon, since no attempt was made to control the sound reflections and interferences which were due to the walls of the room.

II. THEORETICAL DISCUSSION OF THE SOUND INTENSITY REACTION

Introduction.—The attempt to explain theoretically from the behavioristic point of view such a complex activity as that of discriminating between various intensities of tones is undertaken by the writer with considerable hesitation. By some the attempt will be regarded as premature because there is not at present a technique which will permit the unhampered observation of neural function in the normal living organism. How soon such a technique will be developed cannot be foretold. However, if it is possible to get some clear conceptions as to what properties of the nervous system are essential to account for a given form of behavior, it will be easier to determine whether or not these

properties are actually present. That is to say, the writer has set himself the problem of designing a nervous system which if possessed by any organism would enable that organism to react in practically the same way as did the observers in this experiment so far as sound intensity reactions are concerned. In designing such a system it is of course necessary to use anatomical and physiological knowledge that is fairly well established at this time.

Anatomical Basis

The cochlea of the ear is regarded as the organ of hearing. Within this lies the basilar membrane with its rows of hair cells just touching the superposed tectorial membrane. If the relative positions of these membranes are changed, these hair cells are stimulated and a nervous process results. The greater the extent of the area affected the greater will be the number of hair cells stimulated and the greater the total magnitude of the nervous flux generated in the hair cell receptors. It is immaterial for our purpose whether we regard the stimulus as an actual molar movement of the membranes or whether there is only a change in the molecular permeability of the receptors so that concentrations of ions may result and produce the chemical change which initiates the nervous process. In the development of the discussion the assumptions as to the anatomical structure of the ear which are given by Meyer's theory[1] will be used. On the Helmholtz theory of hearing the physiological correlate of intensity is the amplitude of hypothetical fibers in the basilar membrane, resonating to the frequency of the tone in question (in this case 256). Since the longest of these fibers[2] is only 0.3 mm. long and they are blanketed on both sides by white fibrous tissue, it does not seem likely that the twenty or more discriminations made by *Go* could be due to variations of the neural processes brought about solely by changes in amplitude. Under Meyer's theory the intensity of the tone

[1] Meyer, Max F., 'An Introduction to the Mechanics of the Inner Ear,' The University of Missouri Studies, Vol. II. of the Science Series, No. I.

[2] Hardesty, Irving, 'A Model to Illustrate the Probable Action of the Tectorial Membrane,' *Amer. J. Anat.*, 1915, 18, p. 474.

is measured by the relative length of the basilar membrane section which is in motion.

Hardesty reaches the same conclusion but substitutes the tectorial for the basilar membrane. From the fact that discrimination does not vary a great deal for the different intensities, as shown by the averages for each of the intensities of Table II., it seems that the receptors are stimulated in a relatively constant manner.

Hardesty gives the widths of the basilar membrane as 1 : 1.8 for basal and apical ends respectively, and the widths of the tectorial membrane as 1 : 7. This would indicate that the basilar membrane is the more uniform of the two. For our purpose it is not necessary to definitely settle the question as to whether the tectorial or basilar membrane (or possibly both together) is to be regarded the organ which gives the stimulus its characteristic form. Meyer's theory also has the further merit of being the most carefully worked out of any of the theories.

We assume then that the discrimination of sound intensity has as its stimulus the greater or less extent to which the basilar membrane is acted upon and that the action begins at the basal end (the part nearest the oval window) and with increasing intensities extends further and further toward the apical end. It is of course impossible to determine what proportion of the total length of the basilar membrane functioned in this experiment, but for our purpose this is not essential, since we are concerned with the *manner* of function rather than with the range of the action.

Considering the total length which was actually involved as a unit, we found that within this region there were 9 j.n.d.'s and this means that when the difference between any two parts is one ninth of the total length involved, a discrimination is possible. For some individuals (*Wa*) the difference needs to be as great as one sixth, while for a more acute observer (*Go*) one twentieth seems adequate.

From the preceding considerations we may formulate the anatomical conditions under which the intensities of two tones are discriminated as follows:

When the ears are alternately stimulated by differences of tone intensity which are great enough to be discriminated, this involves the alternate vibration of unequal (as to length) sections of the basilar membrane. The greater the intensity the longer will be the section which vibrates. The vibrating area begins at the basal end and extends toward the apical end, so that the receptors for the weak intensities are also stimulated when a stronger intensity acts upon the basilar membrane.

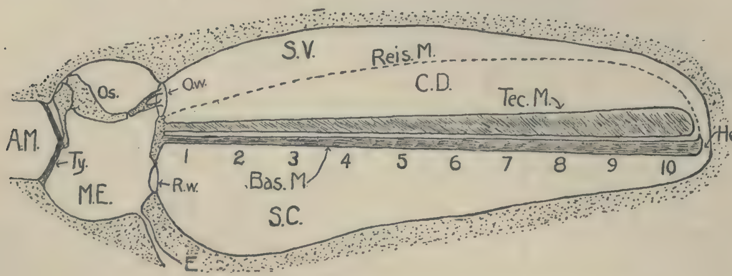


Fig. 3. Diagrammatic representation of auditory function.

A.M., auditory meatus. *Bas.M.*, basilar membrane, including organ of Corti. *C.D.*, cochlear duct. *E*, Eustachian tube. *He*, helicotrema. *M.E.*, middle ear. *Os*, ossicles; malleus, incus, stapes. *O.w.*, oval window. *Reis.M.*, Reissner's membrane. *R.w.*, round window. *S.C.*, scala cochlea. *S.V.*, scala vestibuli. *Tec.M.*, tectorial membrane. *Ty*, tympanic membrane.

In Fig. 3 the structures of the ear are shown diagrammatically. The cochlea is shown uncoiled. The basilar membrane (*Bas.M.*) is shown as a straight membrane upon which rests the tectorial membrane (*Tec.M.*) and the organ of Corti. When a tone of 256 vibrations (for instance) of weak intensity is acting on the ear, the sound waves enter through the auditory meatus (*A.M.*) and set the tympanic membrane (*Ty*) into forced vibrations. These vibrations are taken up by the ossicles (*Os*) and transmitted to the oval window (*O.w.*). In this transmission the amplitude and energy of the vibrations are changed in amplitude but not in form, so that the movement of the oval window may be regarded as a function of the tympanic movement. An inward movement of the oval window increases the pressure

of the lymph in the scala vestibuli (*S.V.*) and this increase in pressure is released by an outward compensatory movement of the round window (*R.w.*). If the basilar membrane were rigid, it would be necessary for the lymph to flow through the opening (*He*). If, however, the basilar membrane is regarded as having a limited flexibility, the relatively great resistance through the tubes *S.V.*, *He*, *S.C.*, make it more probable that the compensation will take the shortest path from the oval window to the round window and thus pass through the membranes *Reis.*, *Tec.*, *Bas.*, and force the round window outward. If the intensity of the tone is weak only the inner sections (1, 2, 3, etc.) nearer the windows will be acted upon. As the intensity of the tone increases additional sections (4, 5, $\cdots$, 10) of the basilar membrane will be affected. For very loud sounds the limited flexibility of the basilar membrane may prove inadequate to compensate for the movement of the oval window, and then there may be an actual displacement of the lymph through the opening *He*. The physical correlate, then, for a tone which gradually increases in intensity is a vibratory movement of the basal regions of the basilar membrane for weak tones, and the successive additions of further sections in an apical direction until with very loud tones the entire basilar membrane may vibrate.

Assumptions as to the Nature of the Receptor-Effector Connection

For the range of the experiment herein reported, it was found that the number of steps which could be discriminated was about nine on the average. This means that if the nine different intensities were presented to the observer in such a way that he could easily compare them it would be possible to arrange them in a series with the weakest and strongest tones at the extremes. The average observer would be able to do this invariably, always getting the same intensity in the same place in the series. Since it is rather difficult to produce tone intensities that can be moved about and arranged as easily as objects such as lifted weights, it is simpler to arrange

the series verbally. That is, by designating the various intensities as 1, 2, 3, · · ·, 10, the average observer should be able (after practice) to designate each intensity correctly. This means that there are nine distinct verbal reactions.

In order to explain how this type of serial activity may be explained on a neurological basis the following assumptions are made as to the manner of stimulation and the way the flux is distributed to the various effectors:

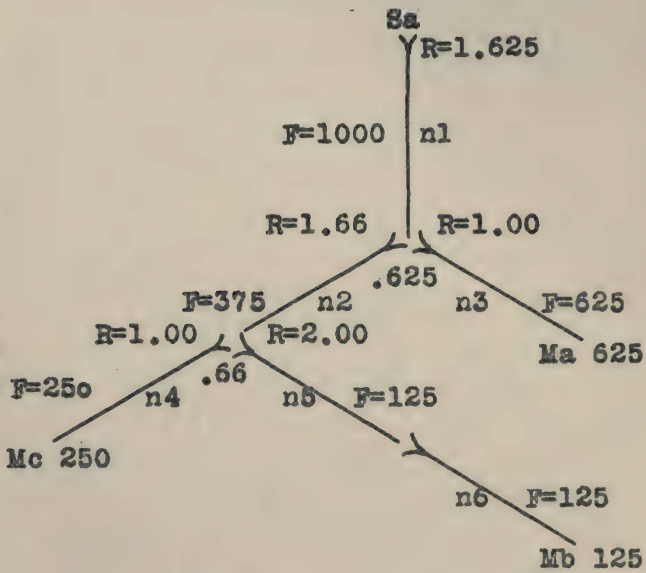


FIG. 4. Diagram showing method of calculating neural function.

1. As the receptors on different regions of the basilar membrane are stimulated, the nervous flux which results is distributed in such a way that at least ten different reactions may take place.

2. The many receptors actually present in the organ of Corti are replaced by only ten receptors distributed in the regions 1 to 10 (Fig. 3). When region 1 only is stimulated it is assumed that the reaction is intensity 1; when both 1 and 2 are stimulated the reaction is intensity 2, etc.

Before developing the hypothetical neural system it is

necessary to consider in some detail just how its properties are to be calculated.

Let $n1$, $n2$, $n3$, $n4$, $n5$, $n6$ (Fig. 4) represent a nervous system of six neurons. There is a single receptor or sensory point at *Sa* and there are in all three motor points or effectors, *Ma*, *Mb*, *Mc*. Each of the six neurons has the same resistance[1] and the direction of the nervous excitation or flux which results when *Sa* is stimulated is indicated by the arrows.[2]

We shall assume that when *Sa* is stimulated 1,000 units of flux are generated and the question then arises what part of this 1,000 units reaches the effector *Ma*, what part reaches *Mb*, and what part reaches *Mc*? In calculating these proportions it is necessary to know the resistance between each effector and the receptor *Sa*. To find these resistances for neurons connected as shown, we use the formula that the reciprocal of the resistance (R) of a system of conductors connected in parallel is equal to $1/R = 1/r' + 1/r'' + \cdots 1/r^n$. In other words the reciprocal of the total resistance is equal to the sum of the reciprocals of the resistances of the parallel branches.

Beginning with *Mc* and counting the number of neurons to the first branching point, we find one neuron, $n4$. Beginning with *Mb* we find two neurons, $n5$, $n6$. We have thus two branches, one of which has a resistance of *one*, and the other a resistance of *two* s.n.[3] Substituting these values for r' and r'' we have $1/R = 1/2 + 1/1 = 3/2$ or $R = .66$. Thus the branch made up of the neurons $n4$, $n5$, $n6$ has a resistance

[1] The term resistance is here merely used in a quantitative sense and refers to the fact that not all the effectors with which a given receptor is neurally connected, receive the same amount of neural flux. Whether we regard the resistance as being a function of the synapse, of the whole fiber, of the cell body or of the rate of metabolic change, is here immaterial.

[2] This is only a diagrammatic representation of the "polarity" principle in neurology in which it is assumed that the nervous impulse passes only in one direction (from receptor to effector) and not in the opposite direction.

[3] s.n. is an abbreviation for "standard neuron." By standard neuron we refer to the resistance of a single independent neuron as represented in the diagram. The total resistance of any system is represented in terms of a nervous system made up of standard neurons connected in series.

which is equal to .66 s.n. The next step is to calculate the resistance of the branch made up of the neurons $n2$; $n4$, $n5$, $n6$; $n3$. The neuron $n2$ has a resistance of 1.00 while $n4$, $n5$, $n6$ were just found to have a resistance of .66 s.n. Adding we have $1.00 + .66 = 1.66$ s.n. for the branch $n2$, $n4$, $n5$, $n6$. The resistance of $n3$ is 1.00 s.n. Substituting 1.00 for r' and 1.66 for r'' in the formula we get $R = .625$ s.n. for the system $n2$, $n3$, $n4$, $n5$, $n6$. Next, neuron $n1$ has a resistance of 1.00 s.n. and the system $n2$ to $n6$ was just found to have a resistance of .625 s.n., so the total resistance of all the neurons in the diagram connected as in Fig. 4 is $1.00 + .625 = 1.625$ s.n.

To calculate the way the flux will be distributed to each of the effectors *Ma*, *Mb*, *Mc*, we begin by the obvious fact that all of the 1,000 units must pass through $n1$. At the end of $n1$ the flux divides, but not equally. The division is inversely proportional to the resistance of each of the paths that are open. These resistances (R) were found to be 1.66 and 1.00, or in the ratio of 5 : 3. For every five units going one way, three go the other. If the 1,000 units are divided in this proportion we have 625 : 375. Since the path which has the least resistance gets the greatest proportion of the flux, $n3$ will get the 625 units and $n2$ the 375 units. The 375 units in $n2$ are then distributed to $n4$, $n5$, $n6$ the division occurring at the end of $n2$ in the proportion of 1 : 2. When 375 is divided into these proportions we have 125 : 250. Of this proportion $n5$, $n6$ get 125 units and $n4$ the 250. From this analysis we can say that the 1,000 units which start at *Sa* are distributed as indicated by the F values opposite each neuron in Fig. 4.

The motor point or effector *Ma* will get 625 units; *Mb*—125 units; *Mc*—250 units. If now *Ma* should happen to represent a system of muscles in the speech mechanism whose contraction results in the pronunciation of the word 'weak'; *Mb* a system whose reaction is 'equal'; *Mc* a system whose reaction is 'strong'; the reaction which would actually take place when *Sa* is stimulated would be the reaction 'weak' since this system of muscles gets more flux than any other.

Having indicated the way in which the hypothetical nervous system may have its properties handled quantitatively we may pass on to a consideration of the development of the serial reaction.[1]

Serial Reaction

The neural system which will meet the requirements of the results of the experiment must account for the following facts:

1. When the difference between the intensities of two tones passes a certain limit, the observer is able to react to each intensity by an independent reaction, such as 'weak,' 'medium,' 'strong,' 'intensity 3,' 'intensity 6,' etc.

2. When the difference between the intensities of two tones is below a certain limit, the reactions lose their independence or consistency. In other words, the intensities may be mistaken.

The above conditions would be met by a system of neurons connected as indicated in Fig. 5.

Fig. 5. Diagrammatic representation of the serial reaction.

In Fig. 5, *S*1 to *Sn* represent sensory points or receptors which are to be imagined as located in the corresponding regions of the basilar membrane in Fig. 2. *M*1 to *Mn* represent motor points or effectors which may be located in various muscle groups such as (1) the speech mechanism by which the intensities are distinguished verbally, or (2) in any other system of muscles by which discrimination can be indicated.

[1] For the mathematically trained reader this presentation will seem too elementary. The formula, however, is somewhat foreign to psychological methods and a detailed application should help some readers to understand the line of reasoning better.

In other words, $M1$, $M2$, $M3$, etc., may represent any muscular activity which in ordinary language is called 'a discrimination.'

In calculating the properties of this system only the part indicated by the solid lines is used. The dotted lines indicate how the system may be indefinitely extended to allow for any number of discriminations. A nervous process starting at S_1 with an intensity of 1,000 units passes to the end of the neuron and there divides; 618 units going to $M1$ and 382 units going into the connecting neuron. Between $S2$ and $M2$ the flux again divides, 236 units going to $M2$ and 146 over the connecting neuron to the junction between $S3$ and $M3$.[1] This continues for the rest of the system, the flux being distributed as indicated. After each division the amount of flux which passes to the connecting neurons is decreased asymptotically, but the reduction of the flux is so rapid that after the eighth effector ($M8$) it is practically zero and may be neglected.[2]

Table III

Stimulation in Isolation

Proportion of flux reaching the various effectors (Mn) from the different receptors (Sn) when the total flux from any receptor is 1,000 units.

Mn	*S1*	*S2*	*S3*	*S4*	*S5*	*S6*	*S7*	*S8*
M1.....	618	...	...	...	...	...	...	...
M2.....	236	618	...	...	...	...	...	...
M3.....	90	236	618	...	...	...	...	...
M4.....	34	90	236	618	...	...	...	...
M5.....	13	34	90	236	618	...	...	...
M6.....	5	13	34	90	236	618	...	...
M7.....	2	5	13	34	90	236	618	...
M8.....	1	2	5	13	34	90	236	618
M9.....	...	1	2	5	13	34	90	236
M10....	...	...	1	2	5	13	34	90
M11....	...	...	...	1	2	5	13	34
M12....	...	...	...	...	1	2	5	13
M13....	...	...	...	...	...	1	2	5
M14....	...	...	...	...	...	...	1	2
M15....	...	...	...	...	...	...	...	1

Table III. gives the quantitative distribution of the flux when the various receptors are thought of as being stimulated

[1] The upper vertical neurons are to be regarded as sensory neurons; the horizontal as connecting neurons; and the lower vertical as motor neurons.

[2] The flux values given in Fig. 5 apply only when the stimulus is $S1$. Other values are given in Table III.

in isolation. Of course, under normal function this is impossible since the stimulation of any receptor always includes the stimulation of the preceding receptors, but to understand Table IV. it is necessary to introduce the concept of isolated stimulation. The values for Table III. are calculated according to the principle used in determining the distribution of the flux in Fig. 4. When this principle is applied to a construction such as Fig. 5 and we are concerned only with the flux which reaches the various effectors we may use the formula of a familiar mathematical series:

$$Mn = (s/2^n)(3 - \sqrt{5})^{n-1}(\sqrt{5} - 1),$$

where Mn is the value of the flux reaching the nth effector and s the value (in this case 1,000) of the initial flux.

From the fact that $M1$ receives such a relatively large proportion of the flux (618/1,000), when $S1$ is stimulated we should expect the reaction (whatever it may be) to occur strongest at $M1$. If $M1$ and $M2$ represent antagonistic reactions such as pronouncing the word 'weaker' for $M1$ and 'stronger' for $M2$, only the reaction 'weaker' will occur since we do not call a tone both 'weaker' and 'stronger' at the same time.

When $S2$ alone is stimulated $M2$ will get the 618 units, while $M1$ does not get any flux. The same principle applies for the remaining construction. In general we may say that the flux is greatest to that effector which corresponds to the receptor; the nearer (functionally) the effector is to the receptor the greater the percentage of flux which reaches it. This type of construction also explains why mistakes are made. If for any reason a neural process coming from outside receptors gets into the system it may change the distribution of flux so that when $S1$ is stimulated a greater proportion of the total flux from all receptors of the body will reach $M2$ instead of $M1$. This manifests itself as distraction or as an error in judgment.

Table IV. indicates the amount of flux which reaches the various effectors when the stimuli are combined so that the stimulus $S3$ includes $S1 + S2 + S3$. The values of Table

IV. are derived by progressively adding the *M* values of Table III. Here again the greatest percentage of the flux reaches the effector which corresponds to the last receptor in the series, but on account of the summation which results

Table IV

Stimulation in Progression

Total flux reaching the various effectors (Mn) from the various receptors (Sn) for various values of n

Mn	Stimulus $\sum_{n=m} Sn$							
	$n=1$	$n=2$	$n=3$	$n=4$	$n=5$	$n=6$	$n=7$	$n=8$
*M*1.....	**618**	618	618	618	618	618	618	618
*M*2.....	236	**854**	854	854	854	854	854	854
*M*3.....	90	326	**944**	944	944	944	944	944
*M*4.....	34	124	360	**978**	978	978	978	978
*M*5.....	13	47	137	375	**991**	991	991	991
*M*6.....	5	18	52	143	378	**996**	996	996
*M*7.....	2	7	20	54	144	380	**998**	998
*M*8.....	1	3	8	21	55	145	381	**999**
*M*9.....	0	1	3	8	21	55	145	381
*M*10....	0	0	1	3	8	21	55	145
*M*11....	0	0	0	1	3	8	21	55
*M*12....	0	0	0	0	1	3	8	21
*M*13....	0	0	0	0	0	1	3	8
*M*14....	0	0	0	0	0	0	1	3
*M*15....	0	0	0	0	0	0	0	1
Total[1] flux...	1,000	2,000	3,000	4,000	5,000	6,000	7,000	8,000

from the combination of the stimuli the magnitudes are greater. Thus when $S1 + S2 + S3$ ($n = 3$) are stimulated the reaction will be at $M3$, since $M3$ gets a greater flux (944) than any other effector. The notation $\sum_{n=m} Sn$ where $m = 6$ means the sum of the amounts of $S1 + S2 + S3 + S4 + S5 + S6$.

The most probable reaction for any series is indicated by the heavy-faced figures. Thus when the stimulus is $n = 4$, the greatest amount of flux (978 units) reaches $M4$, etc. In the serial reaction, Fig. 5, for each receptor there is one effector which receives a greater amount of flux than any other but

[1] *Note:* The sum of the flux values is always a few units less than the "total flux." This is to be expected since we are dealing with an infinite series which only approaches the total flux values as a limit.

the adjacent effectors receive sufficient flux to account for the mistakes or wrong reactions that may be made. As we approach the higher values of n, the differences between the magnitudes of the fluxes which reach adjacent motor points become less and less and approach zero. Thus when $n = 6$, $M4$ gets 978 units; $M5$ gets 991 units; and $M6$ gets 996 units.

It might quite properly be objected that where the differences in flux are so small, we cannot expect a difference in discrimination. That is, when the stimulus is $n = 6$ a reaction at either $M4$, $M5$ or $M6$ is equally probable. This difficulty is due to the simplification which has been necessary to make the diagram easily understood. If each motor neuron in Fig. 5 had been represented as a chain of neurons (a condition which anatomically is much more probable) and if instead of using a regression which practically reaches zero at $n = 9$, we had selected one in which the value does not become less than a mil until $n = 50$, this difficulty would not have arisen.[1]

Until the nature of these series has been experimentally determined we are free to select the one which is most convenient for the purpose in hand.

The Two-way Reaction

We may react to a tone of low intensity by calling it 'weak' and to a tone of strong intensity by calling it 'strong' without making a direct comparison. In other words, we may react to any intensity by the equivalent of either the one or the other speech reaction. This is so well known that experiment is unnecessary, but it is a fact which must be

[1] The following series is such a regression for $n = 51$. The starting point is arbitrarily taken at 100 and every following member is nine-tenths the preceding member:

100, 90, 81, 73, 66, 59, 53, 48, 43, 39, 35, 31, 28, 25, 23, 21, 19, 17, 15, 13, 12. 11, 10, 9, 8, 7, 7, 6, 5, 5, 4, 4, 4, 3, 3, 3, 2, 2, 2, 2, 2, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, Sum = 1,000. Selecting the first 10 or 15 members would avoid the small differences but the reader can easily see that this series treated in the manner indicated in Tables III., IV., V., VI. and Figs. 6 and 7 would extend the tables and diagrams beyond the point of easy comprehension. A *general* algebraical formulation which is simple enough to be easily understood, but yet adequate to give the values of Table VI. for any values of n is beyond the mathematical ability of the writer.

recognized in any theory of the sound intensity reaction. In order to understand this two-way response, another pattern of neural connection must be added to the serial reaction illustrated in Fig. 5. This neurological basis of the two-way reaction is shown in Fig. 6.

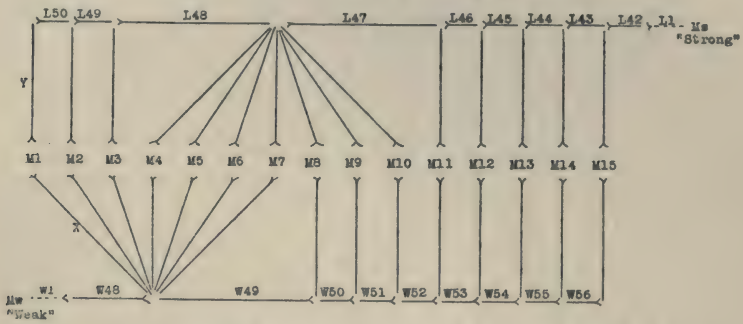


FIG. 6. Diagram of two-way and comparison reaction.

This system has two effectors, one at *Ms* (the speech reaction 'strong') and the other at *Mw* ('weak'). Any flux getting into the system must act on both these effectors but not to equal extent. Whichever receives the greatest proportion of flux will be the only one to react, since ordinarily we do not react to a sound by calling it both 'strong' and 'weak' at the same time. The upper half of the diagram is designated as the 'strong' side. The neurons are indicated by numbers with the prefix '*L*' (loud, rather than '*S*' for 'strong' since '*S*' is already in use for sensory point or receptor). For the 'weak' side or lower half of the diagram the neurons have the prefix '*W*.'

As the values of *n* become higher (the conditions for the higher intensities) the *M* points receiving the highest flux values are displaced more and more toward *Ms*. However, the further they are displaced toward *Ms* the greater the relative proportion of flux which goes to the 'strong' side. Thus at *M*1 the proportion is 50 : 48 in favor of *Mw* whereas at *M*15 it is 55 : 43 in favor of *Ms*. We shall refer to this principle as the displacement of the proportions.

The length of the neural chain at any point to the effectors

Ms or *Mw* is indicated by the numbers of the neurons. Thus *L*45 means that the length and resistance of the chain from *M*12 to *Ms* is to be considered as equal to 45 standard neurons. The justification of this assumption lies in the fact that the reactions 'weak' and 'strong' to sound intensities are acquired rather late in the development of the individual and therefore probably involve a greater number of cortical fibers than the more undifferentiated serial reaction.

The eight effectors *M*1, *M*2, *M*3, etc., of Fig. 5 are to be imagined as entering the two-way system (Fig. 6) at the points *M*1, *M*2, *M*3, etc. Thus the *M*1 of Fig. 5 is to be thought of as ending between the arrows of the neurons *X* and *Y* at the corresponding *M*1 of Fig. 6. The flux at *M*1 divides at this point, part of it goes upward into the system *Y*, *L*50, *L*49, *L*48, $\cdots$, *L*1, *Ms* and part goes downward into the system *X*, *W*48, $\cdots$, *W*1, *Mw*.

From Table IV. we find that when $n = 1$, *M*1 receives 618 units which enters the two-way system at the corresponding *M*1. Since the resistance between *M*1 and *Mw* is taken equal to 48 s.n. and the resistance between *M*1 and *Ms* as 50 s.n. the greater proportion of the 618 units goes to the 'weak' side. However, for the flux from *M*5 (13 units as found in Table IV.) the conditions are reversed and the greater proportion of the flux goes to the 'strong' side since there are only 47 neurons to *Ms* but 48 to *Mw*. The flux at *M*5, however, is less than that at *M*1 (13 as compared with 618) so the reaction still remains 'weak' as will be seen from Table V. where the 'weak' side of the system receives 506 units of flux while the 'strong' side receives 494 units, when all the *M* values given under $n = 1$ in Table IV. have been calculated.

TABLE V

INTRINSIC LOUDNESS OF TONES

Mn	Stimulus $\sum_{n=m} Sn$							
	$n=1$	$n=2$	$n=3$	$n=4$	$n=5$	$n=6$	$n=7$	$n=8$
Mw 'weak'..........	*W*506	*W*505	*W*503	*W*501	*WS*500	499	498	497
Ms 'strong'........	494	495	497	499	*WS*500	*S*501	*S*502	*S*503

In this table is shown the relative amount of flux which reaches the 'weak' and 'strong' effectors when the stimulus conditions are those indicated in Table IV. To simplify the comparison, the total amount of flux has been reduced to a basis of 1,000. It is assumed that the reaction occurs most frequently in that effector which receives the greatest proportion of the 1,000 units. Thus if the flux to 'weak' is 506 units and to 'strong' 494 units, the reaction is 'weak,' which means that the observer will report this intensity as 'weak.'

It may be objected that the differences are so small that it is not likely the reactions are determined by it, but Table V. is merely to show that even when the intensities of tones are judged intrinsically (without reference to a standard) we still call the lower intensities 'weak' and the higher intensities 'strong.'

The values show a gradual change so that low intensities will be called 'weak' loud intensities will be called 'strong' and intermediate degrees ($n = 4$, $n = 5$, $n = 6$) may be called either 'weak' or 'strong.' The differences are nowhere very marked and this corresponds to ordinary experience in that the reactions 'weak' or 'strong' are usually *comparative* judgments, and as such are made with greater constancy. However, under comparative conditions the same intensities may be called 'weak' at one time and 'strong' at another, depending upon the intensity of the tone with which it is being compared.

The Comparison Reaction

The reaction which was recorded in the experiment was the comparison reaction in which the observer was merely asked to report which of two tones was the 'stronger' or 'weaker.' Under these instructions the same intensity may be reported as both 'weak' or 'strong' depending on the intensities with which it is compared. A neural explanation of these reactions must account for the following facts:

1. There are two possible reactions; the observer pronounces either the word 'weaker' or the word 'stronger.' This represents a discrimination.

2. An intensity to which at one time the reaction is 'weaker' will at another time require the reaction 'stronger.'

3. The reliability or consistency of these reactions depends on the difference between the intensities of the two tones which are compared. As the differences become greater the reactions become more consistent.

The comparison reaction can be derived from the same neural structure as the two-way reaction (Fig. 6) but it will be necessary to make an assumption as to the effect of the neural flux upon the resistance of the neurons over which the flux passes. In other words, what are the properties of neural function which make it possible for the flux to distribute itself in one way when the immediately preceding flux resulted in the reaction 'weaker' and in the direct opposite manner when it was 'stronger.'

As an aid in formulating this assumption we may refer to the Ebbinghaus experiments on learning. He found that when a series of nonsense syllables was learned, the rate at which they were forgotten followed a rather definite equation. For a few seconds after learning they were remembered in their entirety. After a few minutes forgetting began to be noticeable and after a half hour a measurable amount had been forgotten. From this we may conclude that while a flux is passing over a neuron, its resistance becomes less and that it remains less for some time and then gradually returns to its original value. We are here concerned only with the resistance conditions for a few seconds after function. We will assume that when the resistance of a neuron has been reduced by function it remains at the reduced level for at least a few seconds. If a second stimulus is ready to go over these neurons while they are in this condition of reduced resistance, a greater percentage of flux will pass over them than would have passed had they been in their original condition. It is this principle which will be used to explain the comparison reaction.

It is not known how much function will reduce the resistance of a neural chain, but for our purpose the exact amount is not significant. We know that within certain limits the greater the flux the greater the reduction in resistance. This is derived from the fact that in memorizing,

the greater the degree of attention the sooner the material may be reproduced. Vivid attention probably means that a relatively strong neural flux is passing over the neurons involved. We will assume that the reduction of the resistance is greatest when the flux is greatest, or that the reduction of the resistance is proportional to the flux. In the absence of quantitative data we shall assume the simplest relationship, namely that the resistance is reduced to the reciprocal of the amount of flux which passes over it. Thus if ten units of flux pass over a standard neuron whose resistance is 1.00, for a few seconds after the flux has passed its resistance will be only one tenth. We can formulate the assumption as follows:

For a few seconds after a neural flux has passed over a neuron its resistance is decreased to the reciprocal of the magnitude of the flux which passed over it.

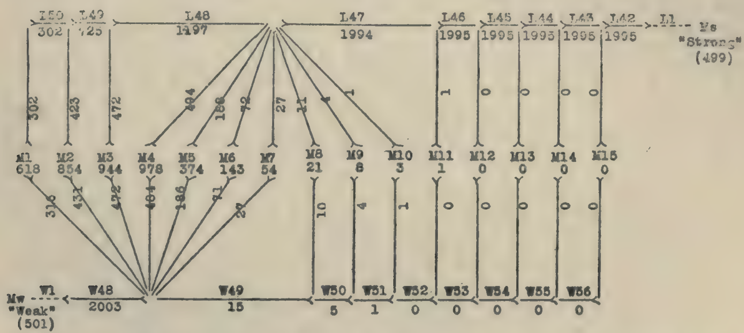


FIG. 7. Diagram of flux distribution for the comparison reaction when $n = 4$.

In Fig. 7 (which is the same construction as Fig. 6) is given the form in which the neural flux distributes itself when the stimulus is $n = 4$ (Table IV.). The total amount of flux is 4,000 units and this is divided among the *M* points according to the values indicated directly under each *M*. The flux reaching any *M* divides into two parts, one part going upward toward the 'strong' side and the other downward toward the 'weak' side. The proportionate amount to each side is indicated on the vertical neurons. The total amount of flux carried by each side is shown on the horizontal neurons under-

neath the neuron number. Thus the number 1,197 below neuron *L*48 means that the total flux which passes through this neuron from *M*1, *M*2, *M*3, is 302 + 423 + 472 = 1,197 units. The same principle applies to the other horizontal neurons.

It will be noted that while the total amount of flux (1,995) which reaches *Ms* is less than the flux (2,003) reaching *Mw*, the neurons on the 'strong' side of the diagram (*L*50, *L*49, *L*48, etc.) actually carry more flux than the neurons (*W*49, *W*50, *W*51, etc.) on the 'weak' side. Referring to the assumption that the resistance of the neurons decreases in proportion to the amount of flux which passes over it, it will be readily seen that the resistance of *L*50, *L*49, *L*48, etc., will be decreased much more than *W*49, *W*50, *W*51, etc. If now the stimulus $n = 5$ follows immediately upon $n = 4$ the resistance through the 'strong' side of the system will be much less than through the 'weak' side and the reaction will be 'stronger,' that is, the observer will judge the $n = 5$ stimulus stronger than $n = 4$. If $n = 3$ had followed $n = 4$ the reaction would have been 'weaker' because the displacement of the proportion of flux distribution favors *Mw*.

Table VI

Value of Flux Reaching *Ms* for Various Temporal Combinations

Std. Intensity	Stimulus $\sum_{n=m} Sn$ Comparison Intensity							
	$n=1$	$n=2$	$n=3$	$n=4$	$n=5$	$n=6$	$n=7$	$n=8$
$n = 1$...	...	*W*494	*S*520	*S*557	*S*601	*S*652	*S*694	*S*730
$n = 2$...	*W*468	...	*WS*500	*S*527	*S*562	*S*605	*S*650	*S*690
$n = 3$...	*W*455	*W*472	...	*S*503	*S*530	*S*565	*S*605	*S*648
$n = 4$...	*W*445	*W*462	*W*472	...	*S*506	*S*533	*S*568	*S*609
$n = 5$...	*W*436	*W*451	*W*465	*W*479	...	*S*514	*S*539	*S*573
$n = 6$...	*W*424	*W*442	*W*456	*W*467	*W*481	...	*S*516	*S*543
$n = 7$...	*W*418	*W*435	*W*448	*W*460	*W*474	*W*486	...	*S*521
$n = 8$...	*W*410	*W*429	*W*444	*W*456	*W*467	*W*480	*W*494	...

In Table VI. are indicated the values of the flux reaching *Ms* for different combinations of stimuli when allowance is made for the reduction in resistance due to the functioning of the flux from the stimulus which was given first. The method of calculating these values is not given in detail

since it would not contribute anything of interest in understanding the sound intensity reaction. Arithmetically the process is simple and of the same type as for Fig. 4.

The stimuli at the left are to be read as coming first and the stimuli at the top are supposed to follow within one second. Thus when $n = 5$ (left) is followed by $n = 3$ (top) we find at the intersection *W*465, which means that 465 parts out of 1,000 units of flux reach *Ms* and since this is less than half of 1,000 the reaction will take place at *Mw* or will be 'weaker.' The prefix letters "*W*" and "*S*" indicate which of the reactions 'stronger' or 'weaker' will probably take place. All magnitudes have been reduced to a basis of 1,000 for easy comparison.

An inspection of the table shows that when the stimulus $n = 1$ (the weakest tone used) is followed by any intensity above $n = 2$ the reaction is 'stronger' and the greater the difference between the intensities the more likely is the 'stronger' reaction to occur, as is indicated by the gradual increase in the magnitudes up to $n = 8$. For all comparisons above the diagonal of blank spaces the reaction is 'stronger'; for all comparisons below, the reaction is 'weaker.' An exception occurs when $n = 1$ is followed by $n = 2$. The reaction to be expected is that of 'stronger' since the intensity of the stimulus $n = 2$ is greater than that of $n = 1$. We find in the table *W*494 which is equivalent to the reaction 'weaker.' This, however, is so close to 500 that we can regard this as an instance of what happens when the difference in the intensities of two tones is subliminal.

The table could of course have been worked out for any number of intensities and an experimental proof of the validity of the assumptions contained in this paper should show that the number of *errors* made in comparing the intensities of two tones are proportional to the magnitudes given in the table. The writer hopes to carry out this experiment. At any rate all the conclusions which may be deduced from the assumptions are open to experimental verification without the interpolation of such conceptions as memory,

reason, imagery, attention, affective tone, or any subjective terms whatever.

In conclusion it may be well to anticipate the criticism that there is no proof that neurons are arranged in the nervous system as indicated in the diagram. Very probably they are not, because the genetic development of the nervous system was not supervised by an engineer. However, other types of connections can be developed that give the same effects as result from these diagrams. The orderly and mechanical arrangement is not necessary for proper function. Crooked lines bearing a sketchy resemblance to the brain and spinal cord might have been used, but clearness in comprehension would not have been augmented.

If human behavior is to be understood as a function of the nervous system it is necessary, at least from the theoretical point of view, to determine the properties of the nervous system. Dissection and post-mortem examination are extremely valuable, but they have their value enhanced when they are directed by a theory which anticipates certain conditions. Anatomical and functional conditions which are expected are more readily found than when left to chance observation. There is of course the danger of stretching the actual observations to 'fit the theory' but the multiplicity of theories which require conflicting observations soon tend to eliminate spurious observations.

The writer only wishes to say that given an organism which has a neural organization such as that indicated by the diagrams and if the properties of the system are those indicated in the assumptions, this organism would be able to discriminate tone intensities in practically the same way as was done by the observers in this experiment. The question, is the human being such an organism and has the nervous system these properties, is a purely experimental problem; not an easy one, to be sure, though one which is no more difficult than any of the more exact problems in theoretical physics or chemistry. At any rate the need for consciousness or mental factors is eliminated and this is an advantage.

SUMMARY AND CONCLUSION

EXPERIMENTAL

1. The individual variations in the acuity of the tone intensity discrimination are less than the individual differences in pitch discrimination.

2. Within the limits of this experiment about 9 intensity steps were, on the average, discriminated. By extending the intensities to include very loud and very weak tones a discrimination of 25 intensity steps should represent about average accomplishment.

3. Two tones which are judged equal by one observer may be recorded as distinctly unequal by another observer.

THEORETICAL

1. The basilar membrane is the anatomical basis of the sound intensity reaction. The greater the intensity of the tone the greater is the area of the basilar membrane which vibrates. The receptors for weak intensities are also stimulated when strong intensities act on the ear.

2. The sound intensity reaction is made up of two types of responses:

(*a*) *The Serial Reaction.*—Neurologically this may be regarded as a continuous system of receptors which, with increasing tone intensity, are stimulated in greater numbers. The receptors are connected with a limited number of effectors in such a way that the nervous flux originating from the stimulation of a definite group of receptors is distributed to any number of effectors, but in greatest proportion to a given group of effectors which may be called the *corresponding* effectors and which upon functioning discriminate the intensity.

(*b*) *The Comparison Reaction.*—Neurologically this may be regarded as a group of neurons interposed between the receptors in the serial reaction and two supplementary effectors. From the central connection of the effectors in the serial reaction neural connections lead to both the effectors 'weak' and 'strong' in such a way that the reduction of resistance due to function and the principle of the displacement of the proportions of flux, will make the temporal order of stimulation a determining factor in the reaction.

DISCUSSION

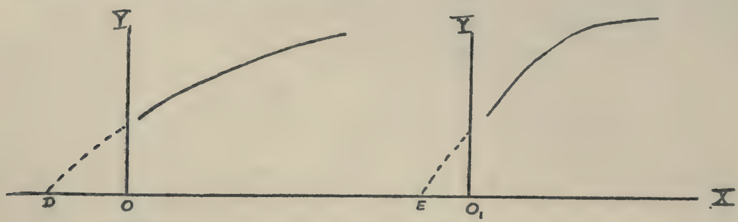
THURSTONE'S METHOD OF STUDY OF THE LEARNING CURVE[1]

Some time ago L. L. Thurstone, of Carnegie Institute of Technology, devised a plan for studying the learning curve that seemed at the time to give the properties of the curve with mathematical accuracy. He represented the general learning curve by the equation $y = a(x + c)/(x + c + b)$. This equation is to be plotted just as any other algebraical equation. The y's are the ordinates and represent amounts of attainment. The x's are abscissas and in Thurstone's work represent practice acts. At the close of the first practice act of the experiment x is given the value 1 and y the corresponding attainment. At the end of the second practice act x is given the value 2 and y the corresponding attainment, etc. In this equation 'a' will be the value of y when x is infinite or will be the physiological limit of attainment; 'c' represents the amount of previous practice or the value of x when y is zero. Thurstone says that 'b' represents the rate of learning. The equation represents the learning curve fairly well. This attempt at an objective method of measuring ability should be a stimulus to further research along this line. Thurstone's equation, however, breaks down in use as follows:

1. *Previous Practice and Physiological Limit.*—Before the experimenter gives his tests the child has already had at different times practice in the function tested in. This previous practice is indeterminate. Possibly a way may yet be found by which the amount of previous practice may be found. Thurstone assumes that the first trial of the experiment is trial 1, or practice act 1. What could a negative amount of previous practice mean? Could there be less than no practice? Suppose that an experimenter practices a child n times in a function, plots the learning curve and solves for the constants. Then a second experimenter (ignorant of the first experiment) takes the same child after the lapse of some time and practices him n times in the same function, plots the curve

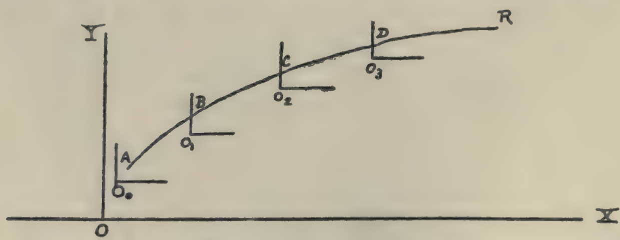
[1] From the Psychological Laboratory of George Peabody College for Teachers. Reference is made to L. L. Thurstone's Paper on 'Calculating Learning Curve Coefficients,' read at the New York meeting of the American Psychological Association. Psychol. Bull., 1917, 14, p. 64.

and solves for the constants. The curve obtained by the second experimenter will begin with less attainment than that with which the first ended. The second curve will rise much more rapidly at the beginning than the first and consequently when completed will show less previous practice than the first curve although we know that the child in the second experiment has had n more previous practice acts than in the first experiment. The physiological limit for the second curve will be found to be less than in the first although it is for the same child and should be the same or a little greater than in the first curve. To illustrate graphically, consider (Curve *a*) and (Curve *b*).



(Curve a) (Curve b)

Let (Curve *a*) be made from the practice of a child in a given function for n practice acts. The previous practice will be represented by the line DO. Let another experimenter (ignorant of the first experiment) after the lapse of some time, practice the child n times in the same function. Suppose that (Curve *b*) is plotted from the data of the second experiment. The previous practice in the latter case will be represented by EO_1. The previous practice in the latter case is less according to the curves than that of the first



(Curve c)

case although we know that the second amount is really greater by n practice acts. The learning curve always rises more rapidly at first after a period of rest. To illustrate still further, consider (Curve *c*).

Take (Curve c), divide it into segments AB, CD, BC, $\cdots$ and considering O_0 the origin of segment AB, O_1 the origin of segment BC, etc., calculate by means of the equation the physiological limits and amounts of previous practice for each of the segments. It will be found that the segments which rise most rapidly will give the greater physiological limits and the segments which are nearer to R will give the lower physiological limits. Now these segments are parts of the same curve AR. If O be assumed to be the true origin and the physiological limit and amount of previous practice be calculated for AR they will be different from those of the segments. This is identically the case of the learning curve. *We do not know the true origin of the curve because we do not know the zero point for practice.*

2. *Rate of Learning.*—The rate of learning is really different for every point on the learning curve. The rate will be the differential of y with respect to x. It will be represented by the expression $ba/(x + c + b)^2$. To compare the rates of learning of two curves at just any random points would mean nothing. Since we do not know the starting point for practice we do not know when we are comparing curves at corresponding points. The constant "b" could not be the mean rate of learning over a certain interval. The mean rate over the interval from $x = n$ to $x = m$ is represented by the expression $ba/(m + c + b)(n + c + b)$. But to compute the mean rate of two curves for the same intervals of practice is not necessarily to get the mean rates for corresponding parts of the curves. The second practice act in the experiment may be the forty-fifth practice act in reality.

3. *Summary.*—The gist of what has been shown is that we cannot get correct values of the learning curve constants by the use of an equation unless we know the true zero-point for practice. In the equation $y = f(x)$ no one should expect to find true values of the constants unless true values of x and y are substituted. This is exactly the difficulty in using any equation for the learning curve. The fundamental problem is to find the true zero-point for practice. All practice must be counted from this if an equation is used.

R. V. Blair.

Nashville, Tenn.

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The Psychological Review

PSYCHOLOGY IN RELATION TO THE WAR[1]

BY MAJOR ROBERT M. YERKES,

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It is my agreeable duty as President of the American Psychological Association to present to you in outline the history of the organizing of psychological military service. I shall limit myself strictly to the activities of the present year, 1917.

In Europe, psychologists have served conspicuously in the great war but psychology has done little. In this country, for the first time in the history of our science, a general organization in the interests of certain ideal and practical aims has been effected. Today American psychology is placing a highly trained and eager personnel at the service of our military organizations. We are acting not individually but collectively on the basis of common training and common faith in the practical value of our work. At the first call American psychologists responded promptly and heartily, therefore the length to which the development of our work has progressed and the measure of service which has been attained.

On April 6, 1917, in connection with a meeting of a group of psychologists known as the "Experimentalists" which was at the time being held in Emerson Hall, Cambridge, Massachusetts, a session was arranged by Messrs. Langfeld and Yerkes with the approval of the chairman of the meeting, Mr. Titchener, for discussion of the relations of psychology to national defense. Captain Bowen, instructor in military

[1] Address of the President before the American Psychological Association, Pittsburgh Meeting, December, 1917.

science and tactics, Harvard University, attended this meeting and made valuable suggestions concerning the possible rôle of psychology. Notable suggestions were made also by Messrs. Bingham, Troland, Langfeld, Doll, Reeves, Burtt, Wells and others. At the conclusion of the discussion, which clearly indicated confidence in the preparedness of psychology for military service, it was moved by Mr. Warren that a committee consisting of Messrs. Yerkes, Bingham and Dodge be appointed to gather information concerning the possible relations of psychology to military affairs and to further the application of psychological methods to military problems.

On the evening of the same day at an informal conference of the members of this committee (Mr. Bingham's place being taken by Mr. Ogden), it was decided that the matter should be placed before the council of the American Psychological Association, so that our national organization, rather than any restricted or local group, might take action. Following this conference, the president of the American Psychological Association prepared the following letter, which on April 7 was dispatched to the members of the council of the association.

Emerson Hall, Cambridge, Mass.
April 6, 1917

To the Council of the American Psychological Association,

Gentlemen: In the present perilous situation, it is obviously desirable that the psychologists of the country act unitedly in the interests of defense. Our knowledge and our methods are of importance to the military service of our country, and it is our duty to coöperate to the fullest extent and immediately toward the increased efficiency of our Army and Navy. Formalities are not in order. We should act at once as a professional group as well as individually.

As president of the American Psychological Association, I apparently have choice of two lines of action: either I may recommend to the council that a special meeting of the association be called at once to consider the general situation, or I may, instead, ask the council to authorize the appointment, by the president, of such committee or committees from the association membership as seem desirable.

After consultation with a number of members of the association,

I have chosen the second alternative, and I hereby request the council's authorization to appoint such necessary and desirable committee or committees.

The duties of any group or groups of members appointed to represent and act for us would evidently consist, first, in gathering all useful information concerning the varied aspects of the actual and possible practical relations of psychology to military affairs; second, to coöperate, as circumstances dictate, with governmental agencies, with the National Council of Defense, with local psychological groups or individuals, and with such other agencies as may develop; third, to further the development and application of methods to the immediate problems of military selection.

Already many of us are working for national defense in our respective communities. It is my thought that this action by our council should, far from interfering with individual initiative, tend to unite us as a professional group in a nation-wide effort to render our professional training serviceable.

I urge you, gentlemen, to give this matter your immediate consideration, and I trust that you will write freely concerning your own activities, plans and opinions, for your advice and suggestions concerning all aspects of the problem will be quite as welcome as your vote on the above recommendation.

Yours faithfully,
ROBERT M. YERKES,
President, American Psychological Association

It was deemed desirable by two members of the council that a meeting be called immediately for consideration of the situation and decision concerning desirable action. Such meeting was called by the president in Philadelphia on the evening of April 21.

In the meantime the writer, in order to obtain pertinent information concerning actual and possible applications of psychology to military problems and, by request, to advise the Military Hospitals' Commission of Canada concerning the use of psychological methods, proceeded to the Dominion of Canada and made careful inquiry concerning psychological activities in Montreal, Ottawa and Toronto. The following memorandum report of observations in Canada was prepared especially for the Council of the American Psychological

Association. It will serve to indicate both the general nature of the writer's inquiries and information and the personal sources of the same.

Memorandum of Trip to Canada, April 10–14, for Consultation with Military Hospitals' Commission Concerning Applications of Psychological Methods to Soldiers

From Boston to Montreal, Tuesday night, April 10. On Wednesday, April 11, called on Professor William Caldwell, McGill University, and after conference with him was introduced to Major Muchleston, with whom I talked concerning the use of psychological methods in examining recruits. Later lunched with Major Muchleston and Captain Robertson at University Club, and discussed with them possible relations of psychology to varied aspects of the military situation. Also, received the opinions of Captain Ross, a returned officer, who had been wounded in France and who was strongly of the opinion that psychologists and psychiatrists should be of great service in the present situation.

Left at four o'clock for Ottawa, arriving about seven-thirty. Was met by Messrs. E. H. Scammell and T. B. Kidner, of the Military Hospitals' Commission staff, and also by Captain Farrar and Doctor Brigham. We later for two hours discussed the organization and psychological relations of the commission.

Thursday morning, April 12, I spent two hours in conference with Doctor Brigham, describing tests in use at the Psychopathic Hospital, Boston, and offering suggestions concerning their application to the military hospitals situation. The remainder of the morning was spent at the offices of the commission, in discussion of various psychological problems with the staff and in the examination of various reports concerning the condition and treatment of incapacitated soldiers.

Lunched at the Rideau Club with Messrs. Scammell, Kidner, Farrar, Brigham and W. L. McKenzie King. From this group I received valuable information. One of the most

impressive things about the conversation was the constant emphasis of the spiritual as contrasted with the material in human life and the evident feeling that for the sake of material development certain essential aspects of the human have been neglected.

The afternoon was spent at the offices of the commission in further analysis of data and study of the relation of the organization to psychological methods and in conference with Senator McClennan, a member of the commission.

Later, Doctors Farrar and Brigham took me to the office of Major Dodd, who as member of the Board of Pension Commissioners, has very evident appreciation of the need of psychological information, but, like most of his colleagues, is unable to obtain the desired data.

The evening was spent with Doctors Farrar and Brigham in further discussion of the relations of psychologists to the problems of mental disease and defect as they appear in soldiers who are discharged from the army.

Lieut.-Colonel Alfred Thompson was absent from Ottawa, but I met and conferred with Captain Chipman and various civilian authorities connected with the Military Hospitals' Commission and obtained printed materials and various typewritten reports.

The general impression which my conferences in Ottawa gave me was that of urgent and rather generally appreciated psychological needs.

At eleven o'clock Doctor Brigham and I left for Toronto. Arriving there in the morning, we got in touch with Captain Ryan, of the Military Hospitals' Commission, and were taken by him to Doctor Clark, of Toronto University, who is superintendent of the university hospital and dean of the medical school. He, after telling us of the psychiatric work in his institution and of his interest in the problem of feeble-mindedness, took us to the psychological laboratory of the University of Toronto, and there introduced us to Mr. Bott, who exhibited and explained to us most varied and interesting forms of home-made apparatus for the reëducation of crippled soldiers. The entire laboratory is given up to this work,

and Mr. Bott, together with a number of assistants, gives practically all of his time to the task of reëducation.

This work is evidently the most important, from the social and economic standpoints, necessitated by the great war, since hundreds of thousands of soldiers are being returned to society partially incapacitated by paralysis or by the loss or injury of members. The relations of psychology to methods of reëducation, and in general to the treatment of these victims of war, are numerous and obviously important, but at present too little is being done in Canada or elsewhere toward utilizing either psychological knowledge or technical psychological methods.

After lunching with Doctor Clark at the university hospital, we were taken by Captain Ryan to visit two military hospitals. During this trip I especially inquired about the examination of recruits for the aviation corps and learned that no especially significant psychological tests are included in the examination.

In the military hospitals, which for the most part are buildings temporarily converted for the purpose, we saw in a general way the facilities for after-care and treatment of the men who are being returned to the Dominion now at the rate of about one hundred per day.

The chief work of the Military Hospitals' Commission, as indicated by its bulletins and other reports, is the proper distribution and medical care of returned soldiers and their vocational reëducation.

At five o'clock Friday, April 13, I left Toronto for Philadelphia. On the way I had opportunity to examine my Canadian materials and notes and to make memoranda concerning my observations.

While in Ottawa the writer received a telegram from Dr. George E. Hale, chairman of the National Research Council, requesting a conference in Philadelphia on April 14. In accordance with this request the president of the association met Dr. Hale and briefly reported to him the action which had been taken by American psychologists and the results of

observations in Canada. President Hale requested that a psychology committee be organized in connection with the National Research Council and that the president of the American Psychological Association act as chairman of the committee and as member of the council.

He further invited the president of the association to attend the semi-annual meeting of the National Research Council in Washington on April 19, as a representative of psychological interests.

At a special meeting of the council of the American Psychological Association which was held on the evening of April 21 and the morning of April 22 in Philadelphia, there were present, in addition to the president, the secretary of the association, Mr. Langfeld, Messrs. Angier, Bingham, Dunlap and Scott of the council, and by invitation Mr. Dodge. Messrs. Carr and Hollingworth were unable to attend. The action of the council upon the original recommendations of the president are recorded in the following minutes of the meeting.

Minutes of Special Meeting of the Council of the American Psychological Association

The president reported his investigations concerning the possibility of the coöperation of psychologists in a scientific capacity in the present emergency. He described his trip to Ottawa, Toronto, and Montreal, where he found the authorities very much interested in the possibility of psychological assistance. His impression was that they realized that they had made a mistake in not using psychological methods for the selection of recruits and for reëducation from the beginning of the war. The president later went to Washington to consult with the National Research Council.

It was voted that the president be instructed to appoint committees from the membership of the American Psychological Association to render to the government of the United States all possible assistance with psychological problems arising from the present military emergency. The following committees were authorized and their chairmen named.[1]

1. Committee on psychological literature relating to military

[1] For membership of committees see "Psychology and National Service," Psychol. Bull., 1917, 14, 259–262.

affairs. A bibliography should be prepared and the important pertinent literature digested, so that the desirable information may be available to individuals who are interested in various lines of service or are undertaking the solution of special problems.

Chairman, Professor Madison Bentley.

2. Committee on the psychological examination of recruits. It is necessary to prepare a plan for this task, to arrange methods of examining, and if arrangements are made with the War Department, to organize a corps of examiners.

Chairman, Professor Robert M. Yerkes.

3. Committee on the selection of men for tasks requiring special aptitude, as for example various kinds of artillery service, signaling, etc. Chairman, Professor E. L. Thorndike.

4. Committee on psychological problems of aviation, including the pertinent literature, the psychological classifications of an aviator, and the relations of these classifications to mechanical problems. Chairman, Doctor H. E. Burtt.

5. Committee on psychological problems of incapacity, including those of shell shock, reëducation, etc.

Chairman, Doctor S. I. Franz.

6. Committee on psychological problems of vocational characteristics and vocational advice. These problems are related to those of reëducation and incapacity.

Chairman, Professor John B. Watson.

7. Committee on recreation in the army and navy.

Chairman, Professor George A. Coe.

8. Committee on the pedagogical and psychological problems of military training and discipline.

Chairman, Professor Charles H. Judd.

9. Committee on problems of motivation in connection with military activities. Chairman, Professor Walter D. Scott.

10. Committee on problems of emotional characteristics, self-control, etc., in their relations to military demands.

Chairman, Professor Robert S. Woodworth.

11. Committee on acoustic problems and characteristics of the sense of hearing in relation to military service; for example, the

significance of localization of sounds, auditory acuity, the possibility of developing ability to discriminate different projectiles by their sound, etc. Chairman, Professor Carl E. Seashore.

12. Committee on problems of vision which have military significance. Chairman, Professor Raymond Dodge.

It was voted that, in order to provide the necessary funds for the development of the plans for national service, the council instruct the president of the association to appoint a special finance committee of three, which shall be empowered to raise and disburse a special fund, without authorization of the council. If the fund is not sufficient, said committee shall be empowered to present itemized bills, not to exceed the sum of one thousand dollars, which the council at the next annual meeting of the association shall recommend to be paid from the funds of the association.

It was moved and seconded that the secretary be instructed to formulate a letter to the members of the association describing the action of the council at this special meeting, in the hope that the members will communicate with the president concerning the best methods of offering their own resources and the resources of their laboratories to the government.

The council made certain suggestions to the president concerning the presentation to the proper government authorities of a plan for the psychological examination of recruits, and authorized the president to proceed with such presentation.

HERBERT S. LANGFELD,
Secretary, American Psychological Association.

Following this council meeting and by authorization already indicated, a psychology committee of the National Research Council was organized with the following membership: Messrs. Cattell, Hall, and Thorndike from the National Academy of Sciences; Messrs. Dodge, Franz and Whipple from the American Psychological Association; and Messrs. Seashore, Watson and Yerkes from the American Association for the Advancement of Science.[1]

At the first meeting of this committee, it was voted "that whereas psychologists in common with other men of science may be able to do invaluable work for national service and in

[1] Mr. Cattell resigned from the committee in October. Messrs. Angell and Scott have been added to the membership.

the conduct of the war, it is recommended by this committee that psychologists volunteer for and be assigned to the work in which their service will be of the greatest use to the nation. In the case of students of psychology, this may involve the completion of the studies on which they are engaged."

It is the function of this general committee to organize and, in a general way, supervise psychological research and service in the present emergency. Problems suggested by military officers or by psychologists are referred by the committee to appropriate individuals or institutions for immediate attention.

Several of the committees originally appointed by the Council of the American Psychological Association were subsequently accepted as subcommittees of the psychology committee of the National Research Council.

After the meeting of the National Research Council which the president of the American Psychological Association was privileged to attend in Washington, and at which he made a brief statement concerning the possible service of psychology to the military organizations, a circular letter was addressed to the members of the American Psychological Association in which their coöperation with the government in the interest of national defense was suggested. It was especially indicated that psychological laboratories might be made available and that offers of personal service would materially assist the council in formulating and furthering plans for the development of national service.

During the last week in April, in pursuance of the suggestions of the council of the American Psychological Association, the president acting as chairman of the committee on methods for the psychological examining of recruits, prepared for transmission to the proper military authorities the following plan for the examining of recruits.

Plan for the Psychological Examining of Recruits to Eliminate the Mentally Unfit

Whereas the Council of the American Psychological Association is convinced that in the present emergency American

psychologists can substantially serve the government under the medical corps of the Army and Navy by examining recruits with respect especially to intellectual deficiency, psychopathic tendencies, nervous instability, and inadequate self-control, it has voted to present to the proper military authorities the following plan and suggestions for psychological service.

This is not intended as a reflection on the work of the military medical examiner, but instead as an offer of special professional aid in a time of unusual strain, pressure and haste. Psychologically incompetent recruits are peculiarly dangerous risks with respect to disaster in action, incapacity, and subsequent pension claims. For this reason and because few medical examiners are trained in the use of modern methods of psychological examining, our profession should be of extreme value to the medical corps.

It is proposed:

1. That the psychological examining be conducted in the training camps and, if possible, before men are finally accepted for the service. It is assumed that there will be ten to fifteen thousand men per camp.

2. That a chief psychological examiner, who shall be also an officer in the Medical Reserve Corps[1] and responsible to the chief medical officer, be located in each camp, to organize, direct and engage in the psychological work.

3. That the chief psychologist be allowed an assistant examiner for each twenty-five hundred men in the camp.

4. That each assistant psychological examiner be given a commission in the Medical Reserve Corps, in order that the psychological work may be conducted with proper decorum and with due respect of private for examiner. The service of civilians as psychological examiners would appear to be undesirable.

5. That properly trained privates, non-commissioned and commissioned officers, be assigned to psychological examining as need requires and given rank in the Medical Reserve Corps.

[1] Since most psychological examiners do not hold medical degrees, it would be necessary probably to commission them as civilian experts under the Medical Corps.

This provides for the contingency that a large number of the men who are especially qualified for this work may have enlisted or accepted commissions in army or navy prior to the organizing of psychological corps.

6. That an appropriate private examining room with floor space of approximately 10 by 12 feet be provided for each psychological examiner. For a camp of ten to fifteen thousand men, five such rooms would be required. A table, flat-top desk with locked drawers, and two or three chairs would be needed in each room.

7. That the necessary materials for psychological examining and the requisite record blanks and filing cards be supplied by the Government. It is estimated that the materials would cost approximately five dollars per examiner, wholesale, and the record sheets not more than a cent per man examined.

8. That the procedure of examining be substantially as follows:

(*a*) In consultation with medical officers and company officers the psychologist should prepare a list of all men in a given company for whom special psychological examination is indicated by exceptional or unsatisfactory behavior.

(*b*) With this special list before him the psychological examiner should summon the men of the company to appear, one at a time, in his examining room. Each should there be subjected to a short series of mental measurements, the necessary time for which should not exceed ten minutes. The result of these measurements should be a rough estimate of the mental status and chief characteristics of the individual and consequent classification as mentally *inferior*, *normal*, or *superior*. Special attention should be given to men whose mental fitness had been questioned by medical or other officers.

(*c*) The normal group (probably 80 to 90 per cent. of all) should be passed, without further examination, as mentally competent. The inferiors should be given a special examination at once to decide whether they should be eliminated from the service. The superiors, time permitting, should be systematically examined for indications of their special value in the military organization.

(*d*) The special examination for inferiors (or superiors) would

require from thirty to sixty minutes. It should consist of measurements of various forms and aspects of response, among which should appear motor characteristics (for example, quickness, steadiness, and fatigability), observation, memory, suggestibility, adaptability or rapidity of learning, judgment, reasoning power, instinctive and emotional traits.

(*e*) The psychological staff should discuss the examination record of each inferior man and vote on the question of recommending to the medical officer his rejection or discharge from the service.

9. Further, this plan should be elaborated and perfected by a competent group of psychological examiners in conference and in coöperation with military authorities.

ROBERT M. YERKES,
Chairman, Psychology Committee,
National Research Council.

Early in May this plan was submitted to the president of the National Research Council, who in turn referred it to the chairman of the Committee on Medicine and Hygiene of the Council, Dr. Victor C. Vaughan. With Dr. Vaughan's support and coöperation the plan was promptly placed before the Surgeon General of the Army.

The evident necessity for developing methods of psychological examining especially adapted to military needs stimulated the chairman of the committee on methods of examining recruits to seek such financial aid as would render possible the organizing of an active committee for this special task. About the middle of May this need and opportunity were brought to the attention of the Committee on Provision for the Feebleminded (Philadelphia), whose secretary, Mr. Joseph P. Byers, immediately presented the matter to his board. It was promptly voted by this organization to offer the committee on methods facilities for work at The Training School, Vineland, New Jersey, and to meet the expenses of the work to an amount not to exceed five hundred dollars. This sum was later increased to seven hundred dollars. On the basis of this offer of assistance, a committee consisting of Messrs. Bingham, Goddard, Haines, Terman, Wells, Whipple

and Yerkes was assembled at The Training School, Vineland, New Jersey, on May 28. It remained in session until June 9 when it adjourned for two weeks to make certain trial of methods which had been devised.

During the first two weeks it was decided to arrange a method of examining recruits in groups of twenty-five to fifty, as an initial psychological survey. The group method, as finally agreed upon and printed for preliminary trial, consists of ten different measurements.

From June 10 to 23, the various members of the committee conducted examinations by the above method in several parts of the country. In all, about four hundred examinations were made, chiefly upon United States Marines and candidates in officers' training camps. These measurements were carefully analyzed by the committee and used as a basis for revision and the devising of methods of scoring.

On June 25, the committee resumed its sessions at Vineland and continued its work until Saturday, July 7, when it adjourned, on the completion of tentative methods of group and individual examining. At this time the committee had in press five forms of group examination record blanks; an individual examination record blank, which provides special forms of measurement for illiterates, those who have difficulty with the English language, those who exhibit irregularities suggestive of psychopathic condition, those who are intellectually subnormal or inferior, and finally, those who are distinctly supernormal; an examiner's guide, which contains directions for the conduct of examinations; and various types of special record sheet.

Before its adjournment, the committee, through a joint committee of psychiatrists and psychologists, consisting of Doctors Copp, Meyer, Williams, Terman, Haines (Bingham, alternate) and Yerkes received assurance from the committee on furnishing hospital units for nervous and mental disorders to the United States government[1] that the above committee would finance to the extent of twenty-five hundred dollars the trial of the above methods of psychological examining in

[1] Mental Hygiene War Work Committee.

various army and navy organizations, the work to be so planned as to test thoroughly the reliability and serviceableness of the methods and to supply materials for their improvement and for the development of satisfactory methods of scoring and reporting data of examinations.

This offer of assistance resulted in the prompt formulation of the following plan, which was successfully carried out.

Outline of Plan for Trial of Methods of Examining Recruits

Five groups of three men each to be organized for immediate work in four different military establishments, each group to consist of a chief examiner and two assistants. The fifth group to be organized for statistical work.

The four examining groups are to work for one month in either naval stations, army reorganization camps, or officers' training camps. It is proposed that approximately one thousand men be examined at each place by the group method, and approximately two hundred by the individual method. Further, that so far as possible, the results of these examinations be correlated with industrial and military records or histories.

This work is to begin as soon after July 15 as possible. Toward the expense $2,500 has been appropriated.

The estimated cost of the work is as follows:

Printing	$ 500.00
Stencils	25.00
Maintenance and travelling expenses of fifteen men, $75 each	1,125.00
Examiners' materials, including lock-boxes for shipment and storage	290.00
Furnishings for four examining rooms	250.00
Total	$2,190.00

Records of examinations are to be shipped to the statistical unit in New York City as rapidly as possible, so that they may be scored and the results evaluated and correlated with a view to determining the best methods of scoring and desirable changes in methods of examining.

Personnel of Units

Examining unit, Fort Benjamin Harrison, Indianapolis, Ind.: Chief examiner, G. M. Whipple, succeeded by T. H. Haines; Assistant examiners, J. E. Anderson, W. K. Layton.

Examining unit, Camp Jackson, Nashville, Tenn.: Chief examiner, E. K. Strong; assistant examiners, B. R. Simpson, D. G. Paterson.

Examining unit, Reorganization Camp, Syracuse, N. Y.: Chief examiner, J. W. Hayes; assistant examiners, J. C. Bell, W. S. Foster.

Examining unit, Naval Training Base No. 6, Brooklyn, N. Y.: Chief examiner, R. S. Woodworth; assistant examiners, N. J. Melville, G. C. Myers.

Statistical unit: Statistician, E. L. Thorndike; assistants, A. S. Otis, L. L. Thurstone.

The examining of approximately four thousand soldiers in accordance with the plan described above and the comparison of the results with officer's ratings of the men revealed correlation of about .5, and in general justified the belief that the new methods would prove serviceable to the Army.

On July 20, after the adjournment of the Committee on Methods and as a direct result of its work, the following substitute plan for the psychological examining of recruits was forwarded to the Surgeon General of the Army.

Plan for the Psychological Examining of Recruits to Eliminate the Mentally Unfit

This replaces a plan earlier submitted, action on which has been postponed by request.

The committee on methods of the psychology committee of the National Research Council, having completed the preparation of methods especially adapted to the selection of a 'first-line army' and the elimination of bad mental risks, respectfully submits the following plan to the Surgeon General of the Army.

I. *Personnel:* It is proposed,

(1) That six (6) qualified experts, designated hereafter as chief psychological examiners, be recommended immediately for commissions in the Sanitary Corps. One chief examiner shall be located in each camp to organize, direct, and participate in the psychological work. (Pending action on commissions, these men might be employed under the Civil Service Commission as provided in (2) below.)

(2) That eighteen (18) men be employed immediately as assistant psychological examiners under the provision of the Civil Service Commission for the employment of unlisted scientific experts. This will not involve delay, since examination is not required.

Supplementary Memorandum concerning Personnel

If this plan should be acted upon favorably, a list of names for commissions in the Sanitary Corps and for civil service employment, will be submitted promptly upon request.

The committee has a list of 150 men who have signified their willingness to serve the government as psychological examiners or investigators. Of these it is estimated that not more than 75 are both properly qualified and able to undertake this work within a month.

It is further estimated, from information which we have at hand, that within a week 24 men can be made available for service as recommended under sections (1) and (2).

II. *Apparatus and Other Equipment:* It is further proposed,

(1) That there be provided in connection with each camp in which psychological examining is to be conducted:

(*a*) One room for group examinations with floor space approximately 28 by 40 feet, and door at each end. The same to be furnished with 56 tablet-arm chairs (or small tables and chairs), and examiner's table.

(*b*) Not fewer than two rooms with floor space at least 10 by 12 feet, to be used as private examining rooms. Each to be furnished with table and three chairs.

(2) That the following apparatus and supplies be furnished:

(*a*) For each examiner:

(1) Cube construction,
(2) Cube imitation,
(3) Cheap clock,
(4) Stop watch,
(5) Pencils and paper,
(6) Examiner's guide or manual
} Estimated cost, \$15

(*b*) For each camp, or examining unit:
(1) Pencil sharpener,
(2) Stock of pencils and paper
} Estimated cost, \$10

(*c*) For each recruit examined:
Group record blank, and in certain instances also individual record blanks. Estimated cost, 2 to 4 cents according to number printed.

Memorandum concerning Examining Outfits

Fifteen complete sets of examining materials are at present in use in various army and navy camps and stations. These can promptly be turned over to the government, in case this plan is acted upon favorably.

III. *Procedure in Examination:* It is proposed,

(1) That all recruits be subjected, in groups of about 50, to a general mental examination which shall require not less than 20 nor more than 40 minutes per group. It is the expectation of the committee that this time can be reduced to 20 minutes. If so, the average time per man would be less than ½ minute.

(2) That all recruits, on the results of the group examinations, be tentatively classified as mentally (*a*) low; (*b*) high; (*c*) average; (*d*) irregular, and that as time permits the lowest 10 per cent., the highest 5 per cent., and irregular individuals shall be subjected to more searching individual examination, on the basis of which, report shall be made by the psychological examiner to the proper medical officer.

(3) That all psychological examining be done under authority of the Army Medical Corps, and immediately under the direction of those medical officers who are specially designated to deal with nervous and mental cases.

(4) That the chief purpose of psychological examination be classification of all recruits as first, second or third class

mentally, so that the War Department may use the data of examination as a basis for elimination or for service classification, as policy or necessity dictate.

Copies of the Examiner's Guide, of Form A of the group examination record blank, of the individual examination record blank, and of a special four-page record sheet, are submitted with this plan. These materials have been printed in several hundred copies, and are now being used in an extensive test of the usefulness of the method of examining recruits which is recommended.

Respectfully submitted,

ROBERT M. YERKES,
Chairman, Psychology Committee,
National Research Council

July 20, 1917

Early in August report of the trial of methods of psychological examinations in Army and Navy stations was prepared and on the basis thereof it became possible definitely to recommend to the Medical Department of the Army official trial in the drafted Army of the methods prepared by the committee.

The chairman of the committee was, upon recommendation of Doctors Vaughan and Welch, of the National Research Council, appointed with the rank of major in the Sanitary Corps, National Army, to organize and direct psychological examining for the Medical Department.

It was later decided by the Medical Department to authorize an initial experiment in the following National Army cantonments: Camp Lee, Petersburg, Virginia; Camp Zachary Taylor, Louisville, Kentucky; Camp Dix, Wrightstown, New Jersey and Camp Devens, Ayer, Massachusetts. Arrangements were made for the necessary personnel and equipment, and instructions to commanding generals of the divisions were issued by the War Department.

THE PSYCHOLOGICAL STAFFS OF THE SEVERAL CANTONMENTS

Camp Lee: First Lieutenants Clarence S. Yoakum, George O. Ferguson, Jr., Walter S. Hunter, Edward S. Jones, and

the following civilians: Leo T. Brueckner, Donald G. Paterson, A. S. Edwards, Rudolph Pintner, Benjamin F. Pittenger, Ben D. Wood.

Camp Taylor: First Lieutenants Marion R. Trabue, Karl T. Waugh, Heber B. Cummings, Edgar A. Doll and the following civilians: James W. Bridges, J. Crosby Chapman, John K. Norton, Eugene C. Rowe, J. David Houser, C. P. Stone.

Camp Dix: First Lieutenants Joseph W. Hayes, Harold A. Richmond, Herschel T. Manuel, Carl C. Brigham, and the following civilians: Thomas H. Haines, Norbert J. Melville, H. P. Shumway, Thomas M. Stokes, J. J. B. Morgan, C. C. Stech.

Camp Devens: First Lieutenants William S. Foster, John E. Anderson, Horace B. English, John T. Metcalf, and the following civilians: Raymond H. Wheeler, Harold C. Bingham, Carl R. Brown, Chester E. Kellogg, Ralph S. Roberts and Charles H. Toll.

The work of psychological examining proceeded rapidly, and in December, on the basis of statistical data and reports of inspectors, it was decided by the Medical Department to recommend the extension of psychological examining to the entire Army.

On December 24, 1917, this recommendation was approved by the General Staff and the Medical Department was requested to present a plan for the examining of all newly appointed officers and all drafted and enlisted men. The Section of Psychology, Office of the Surgeon General is now engaged in preparing a plan for the extension of the work.

Official reports of the results of psychological examining will be issued from time to time by the office of the Surgeon General. In this connection it will suffice to quote briefly from the recommendation of the Medical Department.

"The purposes of psychological tests are: (*a*) to aid in segregating the mentally incompetent, (*b*) to classify men according to their mental capacity, (*c*) to *assist* in selecting competent men for responsible positions.

"In the opinion of this office these reports (accompanying

recommendation) indicate very definitely that the desired results have been achieved.

"The success of this work in a large series of observations, some 5,000 officers and 80,000 men, makes it reasonably certain that similar results may be expected if the system be extended to include the entire enlisted and drafted personnel and all newly appointed officers.

"In view of these considerations I recommend that all company officers, all candidates for officers' training camps and all drafted and enlisted men be required to take the prescribed psychological tests."

The work in question has thus far justified itself in the opinion of military authorities on the following practical counts:

(1) As a means of eliminating certain types of mentally incompetent men; (2) as a prompt and convenient method of obtaining for the use of company commanders information which leads to the intelligent assignment and training of men; (3) as a guide to such assignment of men as will result in organizations approximately equal in mental strength; (4) as an aid in the selection of candidates for officers' training groups, for promotion and for special assignment."

The account of the work of this single committee is inordinately lengthy and for the moment it may tend to give an unjust estimate of the significance of other lines of work which have led to important service. It is now proposed to present briefly an account of the development of certain of the more important of these lines of work. They will be considered under the head of committees.

The committee, which under the chairmanship of Dr. Thorndike was charged with problems concerning the selection of men for tasks requiring special skill, has rendered conspicuous service to the committee on methods of examining recruits and the committee on aviational problems. It further has the special distinction of having indirectly furthered the organization of the Committee on Classification of Personnel of the War Department.

The latter organization is due directly to the activities of Dr. Scott who shortly after the special meeting of the council of the American Psychological Association began work in collaboration with his colleagues of the department of psychology, Carnegie Institute of Technology, Pittsburgh, on the preparation of a rating scale for the selection and promotion of officers. Dr. Scott, during July, by personal representation succeeded in introducing this scale in several of the first series of officers' training camps. The trials of the method resulted in favorable and extremely enthusiastic reports from commanding officers to the War Department, consequently the rating scale in revised form was ordered to use in all of the second series of training camps and the Secretary of War, convinced by results already achieved, of the serviceability of psychologists and psychological methods, requested that a special committee be organized to be known as the Committee on Classification of Personnel of the War Department. This committee was organized early in August, 1917.

The need which led to this development is thus briefly stated in Dr. Scott's response to the request of the Secretary of War.

"The attached memorandum makes it apparent that the chiefs of the War Department Bureaus are not wholly satisfied with present methods of classifying personnel and making recommendation for commissions and that they desire to try out the plan outlined below."

"The first aim of this committee is to bring to the aid of the various bureaus the combined judgment and experience of specialists in methods of employing, examining, rating and classifying men according to their native abilities and their relative value to different branches of the service. The second aim is to provide a means for bringing to a focus the combined experience of the various branches of the Army organization in selecting, classifying and assigning men and in recommending them for commission.

"This double purpose requires that the committee consist of (1) a scientific staff of specialists with civilian status,

and (2) a board of coöperating members consisting of a representative from the War College and one officer designated by each chief of a War Department Bureau who chooses to share in the work of this committee. This plan would call for a staff of ten specialists with assistants and a board of coöperating members consisting of ten Army officers."

Psychologists who have served on this committee are Messrs. Angell, Bingham, Dodge, Scott, Shepard, Strong, Terman, Thorndike, Watson and Yerkes. A brief statement concerning the work of the committee prepared by its director, Dr. Scott, on November 12 follows:

My dear Major Yerkes: The work of the committee on classification of personnel in the Army is of such a nature that it is very difficult to state the exact date on which any work begins and is ended. In fact, practically all of our work has developed gradually and so far as we can see now, will never end. It is therefore difficult to give any report.

The following statement, however, will, in a way, conform with your request of November 6:

1. The Rating Scale was made the form for recommending candidates for promotion in eight of the first series of officers' training camps.

2. In the second series of officers' training camps, a permanent record and a rating scale and a pocket rating card were introduced and made a regular part of the system.

3. For selecting candidates for the third series of officers' training camps, the rating scale, a special form of making reports and a pocket rating card are made the official forms.

4. A personnel department has been established in each of the thirty-one divisions now in the United States. Sixteen civilian experts have been employed by the committee to install the personnel department and these sixteen civilians are coöperating with possibly two thousand officers and enlisted men who are devoting their time to this army work. In some of the divisions, this personnel work is developing very rapidly. This is notably true in the case of Camp Upton, where as many as seventy-two officers and three hundred enlisted men have been employed on the work at the same time.

5. The committee has coöperated in the work of psychological examining which is being conducted in four of the cantonments by

the Surgeon General's Department under the direct supervision of Major Yerkes.

6. The committee is coöperating in the work of selecting men for the Signal Corps.

7. The committee is inaugurating a series of trade tests with the hope of making them a regular part of the work in each of the training camps.

8. The committee has formulated a qualification record card for use of officers in the army.

9. The committee has formulated a plan for securing data concerning the eight million registrants who have not as yet been called to the colors.

10. The committee has formulated a plan for promoting, transferring and demoting officers in the army.

11. The committee is formulating a plan to reduce the paper work in the army.

12. The committee has an office in Room 526, War Building, at which place current data are kept concerning all of the men in the National Army and the National Guard. This information is kept on file for the benefit of the Adjutant General of the Army and the Chief of Staff.

13. Seventy-five thousand, six hundred dollars has been appropriated by the War Department for the use of the committee.

WALTER DILL SCOTT,
Director, Committee on Classification of Personnel in the Army

The committee on classification of personnel is not a direct outgrowth of the activities of the psychology committee of the National Research Council, but has resulted from the individual efforts of Doctors Scott and Thorndike who acted as members of an original committee appointed by the council of the American Psychological Association. The work of the committee, as Dr. Scott's summary report indicates, has attained a scope and magnitude which renders adequate account in this connection impracticable. I have endeavored therefore to give such an outline of the work as may serve as a suitable introduction to the more detailed statement which Dr. Scott will make at our meeting tomorrow and so to interest you in this important psychological service that you may all be present to listen to his address.

The original committee on psychological problems of aviation, began the preparation and trial of tests in April under the chairmanship of Dr. Burtt who was assisted by Doctors Miles and Troland. Subsequently Dr. Burtt, because of the need of a vacation, resigned the chairmanship of the committee and Dr. Stratton was appointed in his stead with Dr. Thorndike as executive secretary. Doctors Watson, Maxfield and McComas were added to the membership of the committee.

A large number of measurements on aviational cadets were made in the ground flying schools in Massachusetts, Pennsylvania and California. It is probable that these measurements will ultimately lead to significant practical application.

Meantime, Professor Watson, who in August became a member of the committee on classification of personnel in the Army, accepted appointment as major in the Signal Corps and was placed on duty in the division of personnel. He has been responsible for the organizing of methods, other than the medical, of the aviational examining boards, and has further been charged with the direction of the work of a group of research psychologists.

His own summary statement concerning this work, which pertains to the effects of high altitude, is presented herewith:

The work of the Oxygen Committee is being carried out under the general supervision of Major Lewis, Major Wilmer, Major Watson and Captain Yandel Henderson.

Preliminary investigations are being undertaken at the Bureau of Mines, American University, Washington. Several men in physiology, psychology, and general medicine have been commissioned and are undertaking the actual experimentation. Captain Dunlap is in immediate charge of the laboratory work in psychology. Captain Schneider is in immediate charge of the work in physiology. On January 1, the work will be continued at Mineola, Long Island, where a small laboratory has been built. At present, the work is being carried out upon re-breathing apparatus. A steel tank which can be exhausted will be used in later experiments. In this way, the factor of barometric pressure can be controlled and the aërial conditions more closely simulated.

Psychological members at present on aviational examining boards are: Messrs. Wells, Bentley, Hamilton, Stratton and Henmon.

The work of the aviational committee and of Major Watson, its military representative, is clearly of great importance but it is utterly impossible in this brief address to report results or even to indicate satisfactorily the directions in which psychologists are contributing and may hope to contribute to the success of aerial warfare.

The committee on psychological literature relating to military affairs consists of the chairman only, Dr. Bentley, who for weeks sought out pertinent publications and forwarded to the sub-committees references and summaries of articles. In this way substantial aid was rendered psychologists who were engaged in the development of new methods and in the attempt to make practical application of existing methods of psychological research to special military problems.

The committees on the psychological problems of incapacity, including those of shell-shock, reëducation, etc., under the chairmanship of Dr. Franz, and on the psychological problems of vocational characteristics and vocational advice under the chairmanship of Dr. Watson were subsequently combined under the chairmanship of Dr. Franz who with the assistance of Doctors Watson and Lashley has furthered the development of reëducational methods and has attempted to coöperate in various ways with the appropriate committees of the Council of Defense and with the Division of Special Hospitals, Office of the Surgeon General.

This field of work is obviously of extreme importance and will doubtless demand the services of many psychologists, among whom women may be numbered, as the war continues and incapacitated men are returned to this country.

Dr. Coe, chairman of the committee on psychological problems of recreation in the Army and Navy, promptly organized his committee and instituted investigations which should lead to the establishment of proper coöperative relations between his group and the various agencies actively

concerned with recreational activities in military stations. Dr. Coe's latest report indicates that his committee is attempting so to formulate psychological problems of military recreation as to decide wisely on lines of work which should be prosecuted immediately for the assistance of the recreational organizations.

Pedagogical and psychological problems of military training and discipline, in the interest of which Dr. Judd was named as chairman of the committee, received no attention because of the chairman's inability to find time for additional work. Dr. Judd finally resigned the chairmanship and the Psychology Committee of the National Research Council elected Dr. Bagley in his place. As yet nore port concerning plans for the organization of the committee or for the prosecution of possible work have been received from Dr. Bagley. It is believed that this committee should be able to render signal service to the War Department by carefully analyzing various forms of military training and discipline and by suggesting new methods or modifications of old methods in the light of pedagogical and psychological knowledge.

The chairman of the committee on problems of motivation of military service, Dr. Scott, has been so far occupied with work on his rating scale and with his duties as director of the committee on classification of personnel that he has been unable to give special attention to the tasks outlined for his original committee. It is hoped that in the future opportunity may be found for special studies of motivation.

Similarly the committee on problems of emotional stability, fear and self-control, whose chairman is Dr. Woodworth, has thus far been unable to find suitable opportunity for practical work. According to the latest statement received from the chairman an attempt is being made to develop methods which shall aid in the elimination of men who are emotionally unstable.

Dr. Seashore's committee on acoustic problems of military importance has recently been invited to advise concerning methods suitable for military use and the chairman has submitted suggestions which promise to be of practical importance.

One of the first psychological problems suggested to the psychology committee of the National Research Council for solution was referred in May to Dr. Dodge, chairman of the committee on visual problems of military significance. This had to do with the needs of the Navy and its prompt and successful solution by Dr. Dodge led to increasingly important demands for his professional services. Much of the work which this committee has furthered is strictly secret. I therefore shall make use of a statement concerning the work of his committee which Dr. Dodge has prepared for me. Otherwise, I might make the mistake of telling something which I do not know!

Navy activities of the Psychology Committee: The Psychology Committee has been of service to various enterprises, two of which, involving more than one of our subcommittees, may not be mentioned at this time.

In addition to these the committee on personnel has prepared a qualification record adapted from the qualifications record of the army to the particular requirements of naval service. This has not yet been officially adopted, but preliminary trials have been ordered to ascertain its defects in operation, if there are any.

For the psychology committee of the National Research Council Dr. Dodge undertook the analysis of the reactions and coördinations which are involved in gun-pointing, for the purpose of devising tests to discover special fitness for the several tasks with a minimum loss of time. From this analysis a set of tests for the fire control party was elaborated with the coöperation of interested naval officers. This is now in practical use in the Atlantic Fleet.

Also with the coöperation of various naval officers he devised a graphic instrument to record the pointing reactions, the accuracy of coördinations in keeping on a moving target, and the accuracy of fire. This instrument was put through a severe trial on two of the major fighting units of the navy. It proved its value and has been elaborated into a robust and fool-proof land training instrument. This has already proved highly satisfactory in one of the training stations and has been ordered reproduced for general use.

Recently the psychology committee of the National Research Council has appointed two additional subcommittees, the functions of which are of such nature that they may not

be discussed without lessening the prospective value of the work of the committees.

The obvious and significant trend of our psychological military work is toward service. Psychologists who develop methods or accumulate information which promises to aid us in winning the war are shortly appointed to positions which give them opportunity to apply their special knowledge effectively. At present our profession is importantly represented in the Department of the Adjutant General, in the Navy, the Signal Corps, the Medical Corps, the Sanitary Corps and the Quartermaster Corps. In some of these bureaus men have been commissioned. In others, psychologists are serving under civil appointment.

As we look ahead and attempt to prophesy future needs in the light of occurrences of the past six months, it is clear that the demand for psychologists and psychological service promises, or threatens, to be overwhelmingly great. In the Sanitary Corps alone, for the conduct of psychological examining, it is safe to assume that at least one hundred and fifty men will be needed for military appointments during 1918. For reëducational work during the same period it is probable that at least one hundred men or women, trained in the use of psychological and educational methods, will be required by special hospitals. In the aviational section of the Signal Corps the chances are that all psychologists properly qualified by scientific training who are available can be utilized to advantage. The Navy makes no personnel demands but it is using to very great advantage the professional advice of psychologists properly qualified to give such advice. The committee on classification of personnel has needs which cannot possibly be met by the available supply of well-trained psychologists. It behooves us therefore to maintain, and if possible improve, the facilities for psychological training in our educational institutions and the quality of instructional work. There should be no attempts at short cuts, but instead capable students should be encouraged to train themselves thoroughly for military positions which demand professional competence in addition to sound common-sense, good judgment and strong personalities.

Our profession has brought to the front the desirability and the possibility of dealing scientifically and effectively with the principal human factors in military organization and activity. It is by no means improbable that the work which has been initiated by the council of the American Psychological Association, by the psychology committee of the National Research Council and by the committee on classification of personnel will ultimately lead to the organization of a new bureau or corps within the military department. This bureau would be responsible for the varied human problems which experimental psychology has revealed to the military authorities and has dealt with in effectively practical ways.

It has been my desire in this historical account of our psychological services to give personal credit so far as possible. I cannot close without making grateful acknowledgment to those who have prepared the way for various lines of psychological work and materially furthered our progress. The list is necessarily incomplete. The authorities of the Military Hospitals' Commission of Canada rendered substantial aid at the outset by revealing needs of psychological assistance. Dr. George E. Hale, chairman of the National Research Council, opened the way to service by recognizing our science and by giving it a place in the Council of Defense. Major Victor C. Vaughan and Major William H. Welch foresaw the possibilities of value in psychological examining and took the risk of recommending psychological service to the Medical Department of the Army. Major Edgar King and Major Pearce Bailey materially assisted in the formulation of plans which made possible the proper coördination of psychological work with military training. Colonel Henry A. Shaw, through his thorough inspection of psychological examining and his personal interest in the extension of psychological methods to the entire Army, hastened the latter achievement and greatly encouraged the psychological staff. Surgeon General Gorgas and his staff hospitably received us and supplied ample opportunities for service.

Finally, it remains for me to say that from the very start

there has been conspicuous enthusiasm for psychological military service and loyalty in the service. The work has been arduous, the discouragements often numerous and serious but in spite of them our various lines of work have been carried forward satisfactorily and in most instances with surprising rapidity. Everyone who has had opportunity to share in the work obviously feels that he has contributed to our military progress and has rendered more substantial service through the application of his professional training than would have been possible in any other line.

AN EXPERIMENT IN EMPLOYMENT PSYCHOLOGY

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The experiment described below was carried on during March and April, 1916. It is the first of an extensive series of such experiments, which is being conducted by the Winchester Repeating Arms Company.

Two types of work were chosen as the field for this experiment. The first was that of shell inspection, the second that of gauging shells for minimum and maximum head-thickness. These two operations were carried on in two long, well-lighted rooms, by about 330 girls. About two thirds of the girls were engaged in inspecting, and one third in gauging. This large number of girls offered the opportunity of making a sufficiently extensive series of observations. The further fact that nearly all of these girls were doing piece work, and that most of them were working on the same kind of shell, made this particular field an ideal one as far as furnishing an objective basis for correlations was concerned. The presence of 'scrap' inspection added still further to the reliability of this objective basis.

The general purpose of the experiment was to discover a set of tests which would guide the employment section in selecting candidates for these two types of work. The more specific purpose at this stage was to discover a set of tests which would show a correlation with the 'output' or 'production' of the girls. If it could be demonstrated that the fastest workers were consistently fastest in certain of the tests and the reverse, then it would be reasonable to assume, subject to further proof or disproof, that these tests could be used by the employment section as a basis for selecting applicants. Moreover, it was also necessary to discover what

standards to set even in those tests which showed a correlation.

The first step, after the field had been decided upon, was the choice of materials or tests. The ideal method here would have been the method of miscellaneous empiricism, but the practical exigencies of the situation made an arbitrary choice of tests necessary. In order to make this arbitrary choice as intelligent as possible, it was necessary first of all to analyze the work of the girls to be tested into its fundamental elements. The work of shell inspecting, for example, was done at a table like an upturned shallow box. Upon this hollow table was dumped a large box of brass shells, not yet loaded, and all of exactly the same kind. The work of each girl was to inspect these shells and throw out those that were defective. In doing this, a girl would first gather up a large handful of shells, as many as could be piled in one hand, being careful to have all of them pointing in the same direction. Then she would put both hands around the shells and turn them all up so as to expose their insides. She would then look down into every shell for dents, scratches, stains, and other very minute defects. When any such defect was discovered, the shell was skillfully extracted from the pile and thrown into one of three or four appropriate scrap boxes. The entire handful was then turned over, and the head of every shell examined for various defects. The shells were then held in a horizontal position on the left hand, and allowed to roll from the pile into the right hand. Each shell in the process of rolling from one hand into the other, exposed its lateral surface and was closely scrutinized for scratches, oil-dents, stains and other defects. The good ones were taken by the right hand and dropped into a pocket at the left side of the table through which they fell into a box below.

An analysis of this operation showed that it required the following qualifications:

1. Good eyesight. The defects to be detected were often so minute as to be indistinguishable to any but the best of eyes. It took the experimenter almost a minute to see one of the most common defects which these girls were required

to notice in an instant. Any weakness of the eyes or marked difference between the two, would be likely to show bad results in the inspection.

2. Keen visual discrimination. Good eyesight is not sufficient. The inspector, looking at a whole handful of shells must, with a few glances, be able to recognize those which are defective and remain oblivious to those which are not.

3. Quick reaction, that is, the ability to extract, as quickly as seen, the defective shell and toss it into the appropriate box.

4. Accuracy of movement, required in picking out the right shell from the closely held handful. This requires a very peculiar kind of deftness, and, in order to facilitate it, many of the girls allow their finger nails to grow to an unusually long and sharp point.

5. Steadiness of attention. The least wavering of the eyes or letting up of the attention is likely to allow some bad shells to slip by or to lengthen the operation.

The above analysis having been made, the next step was to find tests which would be likely to bring out the presence of these qualities. In this connection I wish to acknowledge my indebtedness to Professor H. L. Hollingworth, through whose suggestions concerning the choice of tests the results of this experiment are in a large measure due. In choosing the tests the experimenter had constantly in mind several requirements which their ultimate use in the employment section would make necessary.

1. They would have to be short. The more complex tests and the intricate apparatus used in the psychology laboratory would require too much time for use in an employment bureau.

2. They would have to be easily understood by the person who was taking them, and would therefore have to be of a very simple nature. Obviously, if the tests were to require a considerable amount of instruction and a long period of learning before they showed satisfactory results, they could not be used by an employment bureau. It would be just as well in that case to put the applicant at work and see whether she could learn to do the job.

3. The tests would have to be of fundamental significance. It would not be practicable to devise a set of tests peculiarly adapted to only one type of work, because in that case it would be necessary to have as many sets of tests as there were types of work. Therefore, the tests should be of such nature as to be capable of bringing out basic qualities; not only those involved in the work of inspection but such as would indicate adaptability in other types of work apparently different, but really involving the same fundamental qualities.

4. The tests would have to be of such a nature as to avoid the difficulties arising from the lack of elementary education. This applies only to the use of tests where a school education in English is a non-essential. For instance, two of the best inspectors in the room did very poor work in those tests which required familiarity with English letters. It turned out that these girls were Italian and had only been in this country for a short time. One could hardly speak English and the other had gone to an English school for only three months. It appeared necessary, therefore, to use tests in which this difficulty would be reduced to a minimum.

With these conditions in mind, sixteen different tests were chosen and prepared for preliminary trials. It is not possible or necessary to go into detail on all these tests at this point. However, each test was tried out on a large enough number of girls to give a good indication as to whether it was likely to prove significant. A good deal of time was required for this part of the experiment, but eventually a set of eight tests was selected for the body of the experiment.

An important problem to be settled was the question as to just how these tests should be given. The rooms in which the girls were at work were very noisy due to machines and the handling of thousands of brass shells. Should the girls be tested in this noisy atmosphere or should they be taken off to some quiet place, free from any possible disturbance? In the end, it was decided to give the tests in the work room, on the supposition that if the subjects were left in their regular environment, they would be more likely to show characteristic results in their performance in the tests. A small room

was screened off on one side and this served as a place in which the tests could be given with comparative freedom from intrusion.

Some difficulty had been anticipated in putting the tests into operation, both on the part of the girls to be tested and because of the confusion that might be introduced into the running of the shop. It was feared that the girls might resent the experiment as an infringement upon their personal liberties. However, through perfect frankness in explaining to the girls the exact purpose of the tests, and through the help of the foreman and instructors in removing the air of mystery and suspicion which naturally would surround such an experiment, and through the use of a 'matter of fact' procedure which took each girl's acquiescence for granted but still refrained from the slightest indication of compulsion, it was possible to carry out the entire series of tests without a single unpleasant occurrence. At the outset, an instructor took the girl to be tested from her work and brought her into the room and remained there while the tests were being given; but after a short time, even this precaution was unnecessary. The experimenter became a fixture in the shop and could, without the least embarrassment to the girl, bring her into the experimental room for the tests. Thus, a great deal of time and trouble was spared to the foreman and his assistants.

The eight tests chosen for the body of the experiment were as follows:

1. A simple eyesight test with the use of the Lowell chart.

2. A card sorting test. The subject was given a pack of 49 cards, upon the face of each one of which from 7 to 12 letters were distributed promiscuously. Twenty of the cards contained the letter 'O' and the rest did not. The subject was asked to sort these cards into two piles, those which had 'O' on them and those which did not. The time required for this performance was taken and the number of errors recorded. The object of this test was to bring out the subject's ability to pick out the essential element from a more or less heterogeneous collection of elements, and also, in some measure, to bring out the deftness of the subject in

handling the cards. These cards were so marked and numbered on their reverse side that, after every test, it was possible for the experimenter to sort them back into their original order and to observe the number of mistakes that had been made. In this way it was made possible for every subject to perform this test in exactly the same manner.

3. A modification of the Whipple accuracy test. The subject was required to take a brass-pointed pencil and stick it successively into a series of nine graduated round holes in a brass plate. The speed of the subject's movements was controlled by a metronome set so as to allow thirty trials per minute. The brass-pointed pencil was wired in circuit with the brass plate containing the holes so that, whenever the brass point touched the side of the hole or any part of the brass plate, the circuit was closed; and as soon as the circuit was closed, it produced a click in a telephone receiver which the subject held to her ear. At the start of the test, the subject was instructed to put the brass pencil into each hole in succession until she heard a click in her ear, when she was to start all over again. The experimenter also had a telephone receiver to enable him to follow the subject's performance. The holes were numbered 1, 2, 3, etc., to 9. As soon as the subject failed to put the pointer squarely into a hole but touched the brass plate and produced a click, the experimenter recorded the number of the hole at which she had failed. This constituted one trial. Each girl was allowed fifteen such trials, and the numbers of the last ten were taken and averaged, the first five serving as preliminary practice. For instance, if a girl, in her first trial, reached the fourth hole and missed on the fifth, the number five was recorded; if she missed on the sixth, six was recorded, etc., until fifteen numbers had been taken. Then, the sum of the last ten trials divided by ten, gave the average performance for the subject. The larger the average, the better the performance. This test occupied from two to three minutes.

4. The Whipple steadiness test, also modified. The apparatus for the accuracy test and steadiness test was set up as a single piece and wired so as to enable the subject

and observer to detect the contact by means of telephone receivers. This method was found to be much more delicate and reliable than the buzzer method. In each case, fifteen trials were taken. The points reached before contact in the last ten trials were averaged and constituted the record for the test.

5. The Woodworth-Wells cancellation test. The subject was requested to cross out, with a pencil, every '7.'

6. The Woodworth-Wells 'Easy Directions' test.

7. The Woodworth-Wells number checking test, in which the subject was asked to place a check opposite every group which contained both a '7' and a '1.'

8. A modification of the tapping test, in which the subject was requested to push down, as rapidly as possible, a telegraph key to which was attached a Veeder counter. The number of recorded thrusts over a period of one minute constituted the record for that performance.

These eight tests, including the time required to take a girl from her work and back to it, occupied a period of not more than twenty-five minutes.

However, before the tests were given, the subject was questioned as to her age, the length of time she had been at her present work, her previous work, how long she had worked on day rates before being given piece work, also the extent of her education and the nationality of her parents. The employment section had already furnished cards containing a large part of this information, and so it was only necessary to supplement what was already at hand. The questions, however, also served the very useful purpose of decreasing the initial nervousness of the subject, and thus prepared the way for the tests proper.

Seventy-three girls were tested, 52 of whom were inspectors, and twenty-one gaugers. The work of gauging will be described later. It was impossible to test a larger number of girls because the experiment came at a time when the work of shell inspection was rapidly slowing up and a majority of the girls were laid off or transferred to another job.

In determining the results of the tests, the first step was

to obtain the ranking of the girls as shown by their daily work. The experimenter had, while conducting the tests, also kept a record of the number of pounds of shells inspected by each girl on the day that she was tested. However, this record was not deemed extensive enough to afford a reliable criterion of a girl's productiveness. To be sure, if a girl's work on the day that she was taking the tests was unusually high, that fact might show up in an unusually good performance in the tests, and thus serve to maintain the correspondence between the two. However, the object of the test was such as to make an immediate correspondence a distinctly minor feature. It was rather to discover whether any correspondence existed between the performance in certain tests given for the first time and occupying only a few minutes, and the work of a girl over an extended period of weeks and even months. Unless such a correspondence could be shown, the tests would be of little worth to the employment section. Therefore, it was decided to take as the basis of the girls' standing, an average of their work for four weeks. These averages were obtained by making a detailed statement, drawn from the separate daily production slips of each girl, showing the number of pounds done for every day in the week, together with the exact number of hours taken to do them. The total number of pounds inspected by each girl divided by the total number of hours worked gave the average number of pounds per hour for the particular girl and became the basis for her ranking. In making out these statements, all work other than that on a single kind of shell was discarded. This was done for the sake of uniformity, it being manifestly unfair to judge the relative speed of different girls on a basis of pounds when one girl was inspecting large shells which went very fast while another was inspecting small shells which went very slowly.

After the average hourly production for a period of four weeks had been determined for each girl, the performances in each one of the tests were compared with the production rates to determine the correlation which existed between them. This was done by the method of rank-differences using the formula

$$R = 1 - \frac{6\Sigma D^2}{n(n^2 - 1)}.$$

The Pearson product-moments method was used to check the results obtained in the case of the three tests which showed the highest correlation. The difference in the results was negligible. The tests themselves were treated on the basis of speed only, corrections for mistakes being made in terms of time. The Correlations found were as follows:

	C. of C.	P. E.
Card-sorting test	.555	.069
Accuracy test	.377	.085
Steadiness test	.238	.092
Tapping test	.135	.096
Cancellation test	.635	.059
Easy directions	.141	.093
Number-group checking test	.734	.045

Eye test: Because of the nature of the test, only individual correspondences could be shown. Some of these will be mentioned later. It was plainly evident, however, that an inspector required two very good eyes in order to succeed at this work.

From the above figures it will be seen that all the correlations in the case of these eight selected tests are positive, and that three or four of them are quite high.

On the other hand, when the same tests were compared with the work of twenty-one gaugers in the same thorough way, the results were quite different. We may well attribute this difference to the difference in the work. The work of the gauger does not require the use of the eyes to any extent. The operator simply picks up a handful of shells and, with or without looking, tries the head of each shell on a gauge containing two openings. The shells which are too small pass through the first opening into a reject box below. Those that are too large fail to pass through the second opening and are thrown into another box at one side. The operators sit in front of their gauge, which is rigidly fixed, and try each shell at one opening and then at the other, just as rapidly as they can move their hands up and down. It will readily be seen that this work requires capacities quite different from

those needed in the work of inspecting. This difference is fairly well indicated by the differences in the coefficients of correlation found for the two types of work and placed side by side below:

	Inspectors		Gaugers	
	C. of C.	P. E.	C. of C.	P. E.
Card-sorting test	.555	.069	.053	
Accuracy test	.377	.085	.213	
Steadiness test	.238	.092	.258	
Tapping test	.135	.096	.516	.071
Cancellation test	.635	.059	.167	
Easy directions	.141	.093	.183	
Number-group checking test	.734	.045	−.187	

It so happens that the very test which shows the highest correlation among inspectors shows the lowest correlation, a minus correlation in fact, among the gaugers. This is quite in accord with the apparent fact that for the work of inspection, visual discrimination is probably the most important qualification, while for the work of gauging it is probably the least necessary. The only test which shows a significant correlation among gaugers is the tapping test. This seems reasonable since in both the test and the operation of gauging, speed of movement and endurance are the chief factors. The significance of the other tests is chiefly negative since they serve to bring out the fact that girls who to the ordinary observer and even to the trained employment manager look very much alike, may still possess very different sets of qualifications. If all the gaugers and inspectors had been lined up before the employment window, it is highly improbable that the employment manager would have been able by mere observation to make the radical distinction between the applicants which the tests would have enabled him to make.

In order further to prove or disprove the results found in the first section of this experiment, the three tests which were found to show the highest correlation for inspectors were given to twenty-eight cartridge inspectors, ten bullet inspectors and 30 paper shot shell inspectors. A summary of all the correlations for these three tests is given below.

Kind of Work	No. Girls	Card Sort. Test		Canc. Test		No. Group Checking	
		C. of C.	P. E.	C. of C.	P. E.	C. of C.	P. E.
Shell inspecting	51	.55	.07	.63	.06	.72	.04
Bullet inspecting	28	.49	.10	.26	.13	.58	.10
Cartridge inspecting	10	.52	.16	.48	.17	.62	.16
P. S. S. inspecting	30	.13	.12	.11	.13	.02	.13

From these figures it can be seen that the correlations for the cartridge and bullet inspectors come quite close to those found for the shell inspectors but are not quite so high. However, when these tests were being given to the cartridge and bullet inspectors about four fifths of the girls had been laid off and those who remained were, on the whole, girls whose speed was in about the same class. The large individual differences, therefore, which appeared in the work of the shell inspectors were not present and this may account for the slightly lower correlations found on these jobs. The average individual difference or the A.D. from the average hourly production for each of the entire groups was as follows:

	Per Cent.
1. Shell inspectors	27.7
2. Bullet inspectors	18.7
3. Cartridge inspectors	6.9
4. P. S. S. inspectors	2.2

It will be seen from these figures that there is a direct relation between the degree of correlation for these tests and the amount of average deviation for each of the four classes of workers. Through the reduction of the working force, the extremes were eliminated at both ends—the slow girls being discharged, and the very fast ones modifying their pace to suit the retarded pace of production. Therefore, although the individual differences still appeared in the tests, their absence in production rates made it impossible to obtain as fair a comparison between the performance of the girls in the tests and their actual ability as workers as was obtained in the first part of the experiment.

However, the results in their entirety were such as to justify the use of the three most significant tests in the employment section. The employment section is giving these

tests to all applicants for the work of inspecting described above. So far, 78 girls have been chosen on the basis of these tests. The results of these choices are being carefully watched. As soon as production figures for the new girls are available, the tests given will again be correlated with the average hourly output of the girls. In this way it will be possible to discover whether the correlations previously found continue to exist under the new conditions imposed by the mechanism of employment.

ASSOCIATIVE AIDS: I. THEIR RELATION TO LEARNING, RETENTION, AND OTHER ASSOCIATIONS

BY H. B. REED

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The particular problems of this experiment center about (1) the relation of associative aids to the rate of learning and of forgetting; (2) their relation to other associations; (3) their relation to the transfer of learning; (4) their relation to repeated learning or practice; (5) their relation to thinking and the theory of thought; and (6) their relation to methodology in psychology.

The material used in this experiment consisted of the following pairs of words or syllables:

Series *SB*

1.	sauce	balloon
2.	mistake	clean
3.	elephant	steeple
4.	simmer	tarry
5.	ring	kitten
6.	turkey	among
7.	chest	muffin
8.	galley	time
9.	kimono	stencil
10.	space	betide

Series *RC*

1.	radish	coffee
2.	serve	hold
3.	pork	cocoa
4.	cheese	tomato
5.	speak	weigh
6.	ribbon	banana

7. being	credit
8. celery	wafer
9. troll	blast
10. soup	wall

Series *WS*

1. Wipfel	summit
2. beharren	preserve
3. Korper	body
4. anregen	stimulate
5. Dach	roof
6. ereignen	happen
7. Angriff	attack
8. besonder	particular
9. Citrone	lemon
10. geigen	fiddle

Series *WP*

1. wum	pir
2. gab	lek
3. vif	gom
4. mif	jas
5. heb	tup
6. beq	nog
7. zis	ruc
8. paf	zal
9. dev	cux
10. lir	wap

The experimenter took one series at a time, read it once to the subject, and then following the prompting method, required the subject to respond with the second word of each pair as soon as possible after he gave the first word. There were 27 subjects. With the exception of nine, each subject was asked after he had learned the series to report any associations that he had formed between a given pair. The subjects of the exception were asked after every response to tell of what they had thought between the stimulus and the response.

Before beginning the first series, the subject was instructed as follows: "I am going to read some pairs of disconnected words to you like book-ditch or feel-reply. I want you to repeat them after me the first time I read them. The second time I am going to say only the first word of each pair, and I want you to name the second word. If you don't remember it, just say 'No,' and I will prompt you. After you have learned the words, I am going to ask you to tell me whether or not you thought of anything by which you connected the pairs. For example, with book-ditch you might think of a book lying in a ditch, or just a red book, or you might have thought of nothing in particular. In that case, you just tell me that you didn't think of anything." The experimenter gave the stimulus words in the order given above except when a word was correctly responded to in a test. In that case, he skipped that word the next time he went through the series. He kept a record of every wrong reaction, of every reaction time as it was measured by a stop watch, of the number of promptings for each pair, and of every association.

The subject's retention was tested for the four series in the manner described every day for six successive days. On the sixth day three additional tests were given, the B, D, and U tests. In the B test, the subject had to learn one or two series backwards, *i. e.*, name the first word of a pair after the experimenter gave the second. In the D test, the subject had to learn the first word of a series downwards, *i. e.*, name the first word of a pair after the first word of the pair given by the experimenter. The U test was just the reverse of the D test, *e. g.*, when the experimenter gave the first word of the last pair, the subject had to name the first word of the pair just before it. Verbal reports of these tests were required just as in the regular tests.

I

This paper will be limited to the first two problems stated above. Since the relation of associative aids to the rate of learning and of forgetting cannot be conveniently discussed

separately, we shall discuss both of them together. By an associative aid, I understand some association that either ties the words of a pair together for the learner or ties one of them to some part of his experience. In the example given above, the thought of a 'book' lying in a ditch ties the words book and ditch together, and the thought of a red book ties the word 'book' to a part of the learner's experience.

To discover the relation of associative aids to the rate of learning and of forgetting, we might first discover how these two factors are related in themselves and afterwards hunt for a connection between this relationship and the associative aids. The relation between the rates of learning and forgetting may be discovered in three ways: (1) by seriating the average learning times, L.T.'s, of the pairs of each series for the first day of the different subjects in their order of quickness of learning and then finding the relation of the averages of the four quarters to the average L.T.'s of the corresponding quarters twenty-four hours later; (2) by setting up arbitrary limits for quickly and slowly learned pairs and then finding the relation of the L.T.'s of these pairs to the corresponding L.T.'s twenty-four hours later; (3) by setting up arbitrary limits for quickly and slowly forgotten pairs, and then finding the relation of the L.T.'s of these pairs to the L.T.'s of the corresponding pairs twenty-four hours earlier. I have followed the three methods to some extent but have made my principal calculations according to the third method.

In Table I. are presented the results according to the first method. The second and third quarters are averaged into the middle half. It will be seen that if retention is measured by the Saving Method, the old saying 'Quickly learned, quickly forgotten' is the conclusion.

The amount forgotten is in direct proportion to the quickness of learning. However, we may measure retention by the absolute length of the learning time, and in this case the amount forgotten varies inversely as the quickness of learning. To determine the relation between the rate of learning and the rate of forgetting we must first decide which is the better method of measuring retention.

TABLE I

	S.B.					R.C.				
	First Day	Second Day	Per Cent. S.	N.	N.S.	First Day	Second Day	Per Cent. S.	N.	N.S.
Ave. L.T. of first quarter......	3.07	2.73	11	60	6	2.45	2.28	7	50	5
Ave. L.T. of mid. half........	6.57	4.18	36	130	13	4.52	3.48	23	120	12
Ave. L.T. of fourth quarter....	14.16	6.59	53	60	6	10.19	6.21	39	50	5
	W.S.					**W.P.**				
	First Day	**Second Day**	**Per Cent. S.**	**N.**	**N.S.**	**First Day**	**Second Day**	**Per Cent. S.**	**N.**	**N.S.**
Ave. L.T. of first quarter......	5.49	3.40	38	70	7	8.96	4.97	44	70	7
Ave. L.T. of mid. half........	10.44	5.85	44	130	13	15.59	7.41	52	130	13
Ave. L.T. of fourth quarter....	19.02	10.12	46	70	7	31.15	12.21	61	70	7

The L.T.'s are in seconds; Per cent. S. = Per cent. saved; N. = No. of pairs; N.S. = No. of subjects.

The Saving Method furnishes a percentile measure of the amount of repetitions or the time saved in learning, but this does not necessarily correspond with the amount retained. Let us say that *A* learns a stanza of poetry in 2 minutes and relearns it in 1 minute. *B* learns the same stanza in 10 minutes and relearns it in 3 minutes. *A* saved 50 per cent. and *B* 70 per cent. *A* will reproduce three times as much in the same unit of time as *B* and the entire passage in ⅓ of *B*'s time. Shall we then say that he forgot more than *B* because he saved 20 per cent. less of his original learning time? If *A* forgot more than *B*, how could he reproduce three times as much in the same unit of time? If we wish to measure the efficiency of retention, it is evident that we must limit ourselves to the amount reproduced per unit of time. The same rule applies if we wish to compare the efficiency of one subject's retention with another's. In both of these cases, the Saving Method is misleading, and in every case where the learning times are not constant. Its value depends upon the amount of difference between the learning and relearning times in relation to the learning time. This value according to Table I. varies directly with the quickness of learning and relearning, and therefore inversely as the

amount reproduced per unit of time in the retention test. It appears, therefore, not to be a good measure of retention. If so, the facts of Table I. mean that the rate of forgetting varies inversely as the rate of learning. But if it is doubtful what we mean by 'forgetting' or 'retention,' we can at least say that the rate of relearning varies directly with the rate of learning. From the standpoint of efficiency both statements mean the same thing, and, for our purpose, that is sufficient.

The effect of associative aids upon the relation of the rate of learning and of forgetting could be ascertained in part by finding the per cent. of the pairs for the subjects in the first quarter, middle half, and fourth quarter, respectively, that were learned by the aid of association. I have not made this calculation because of the composition of these averages which distribute the individuals only with respect to their rates of learning. But each individual has some pairs which were much more slowly learned than others, some pairs that were learned associatively, and some pairs that were learned mechanically, *i. e.*, without association. The mixture of these quantitative and qualitative elements in one average destroys the true quantitative relationship between the rate of learning and of forgetting and covers up the effects of the qualitative modes of learning, *i. e.*, of the associative aids. The effect of the latter is much more readily discovered if we first sort out the quickly and slowly learned pairs, as in the second method, or the quickly and slowly forgotten pairs, as in the third method.

The second and third methods are probably of equal value, but I have principally followed the third method since it was very easy to set up an arbitrary limit between quick and slow forgetting. I called those pairs slowly forgotten, S.F., which were responded to correctly in the second day without prompting, and those that required one or more promptings I called quickly forgotten, Q.F. I then averaged the L.T.'s of the first and second day for these two sorts of pairs for each subject, then seriated the subjects in their order of quickness of learning for the first day for each sort of pairs,

and found the averages for the first quarter, the middle half, and the fourth quarter, respectively, as in the first method. The results for the four series appear in Table II., which also contains the percentile values of retention, the number of pairs, and the number of subjects for each quarter or half.

Table II. shows that the S.F. pairs, those responded to correctly on the second day without prompting, were quickly learned on the first day, *i. e.*, that quick relearning is accompanied by quick learning. This is evident from a comparison of the L.T.'s of the Q.F. and S.F. pairs, quarter for quarter and half for half for each series. With a few exceptions, we can again say that the rate of relearning varies directly as the rate of learning, also that retention as measured by the Saving Method varies directly as the length of the learning time for both Q.F. and S.F. pairs, and for each kind of material. But the striking fact is that quarter for quarter and half for half the retention for the S.F. pairs is invariably higher than for the Q.F. pairs. Whether we measure retention by the Saving Method or by the length of the learning time, it is higher in every case for the quickly learned than for the slowly learned pairs. The old saying 'Quickly learned, quickly forgotten' must therefore be changed into 'Quickly learned, slowly forgotten.' This relationship is also affected by the quality of the material. The often repeated statement that meaningful material is forgotten more slowly than nonsense material must be qualified in several ways: First, with respect to the Q.F. pairs, the more meaningful material, the more rapid the forgetting; second, with respect to the S.F. pairs, the more meaningful the material, the slower the forgetting. These statements are based on the average per cent. S. for the Q.F. and S.F. pairs of each series.

We are now in position to state the relation of associative aids to the rate of learning and forgetting. This was determined by calculating the number and the per cent. of Q.F. pairs and of S.F. pairs that were learned with an association on the first and second days, respectively. The results are given in Table III. In this table, we notice first the large differences between the Q.F. and S.F. pairs that were learned

TABLE II

	S.B.										R.C.									
	Q.F.					S.F.					Q.F.					S.F.				
	First Day	Second Day	Per Cent. S.	N.	N.S.	First Day	Second Day	Per Cent. S.	N.	N.S.	First Day	Second Day	Per Cent. S.	N.	N. S.	First Day	Second Day	Per Cent. S	N.	N.S.
Ave L.T. of first quarter....	3.40	6.23	−81	26	6	2.06	2.43	−13	37	6	2.05	5.01	−144	11	5	2.27	2.18	4	31	5
Ave L.T. of mid. half......	8.47	14.07	−66	48	11	4.23	2.20	48	71	11	5.58	5.64	−1	42	11	4.31	2.22	48	82	11
Ave L.T. of fourth quarter..	15.03	8.17	45	27	6	9.47	2.37	72	41	6	14.56	5.76	60	18	5	8.84	3.32	62	36	5
Ave. L.T. of all...........	8.86	7.43	16	101	23	4.99	2.30	54	141	23	6.94	5.52	20	71	21	4.90	2.38	51	149	21
A.D..................	3.72	2.50				2.22	.91				2.17	2.29				2.11	.72			
	W.S.										W.P.									
Ave. L.T. of first quarter...	6.01	5.38	10	35	7	1.78	1.67	6	34	7	9.54	6.00	37	46	7	4.93	1.79	63	16	6
Ave. L.T. of mid. half......	14.06	10.75	23	62	13	4.83	1.79	63	56	13	17.06	9.90	42	85	13	13.50	4.85	65	56	13
Ave. L.T. of second quarter.	22.95	12.50	41	42	7	11.35	2.26	71	36	7	33.69	16.15	52	49	7	27.87	2.93	89	16	6
Ave. L.T. of all...........	14.43	9.83	32	139	27	5.76	1.87	67	126	27	19.42	10.56	451	80	27	14.88	2.68	82	88	25
A.D..................	4.92	5.17				3.40	.43				7.65	4.58				6.88	.96			

Q.F. = Quickly forgotten pairs.
S.F. = Slowly forgotten pairs.

Table III

	S.B.							R.C.							W.S.							W.P.						
	Q.F.	a_1	a_2	af	S.F.	a_1	a_2	Q.F.	a_1	a_2	af	S.F.	a_1	a_2	Q.F.	a_1	a_2	af	S.F.	a_1	a_2	Q.F.	a_1	a_2	af	S.F.	a_1	a_2
First quarter	26	11	1	1	37	32	33	11	7	2	1	31	28	23	35	7	0	0	34	31	24	46	2			16	9	7
Mid. half	48	23	6	5	71	61	43	42	26	3	3	82	59	56	62	21	6	2	56	39	33	85	26	2	1	56	40	34
Fourth quarter	27	5	1	1	41	37	33	18	11			36	31	26	42	7	2	1	36	27	19	46	16	3	3	16	11	14
Totals	101	39	8	7	149	130	109	71	44	5	4	149	118	105	139	35	8	3	126	97	76	180	44	5	4	88	60	55

a_1 and a_2 Expressed in Percentages

	Q.F.	a_1	a_2	af	S.F.	a_1	a_2	Q.F.	a_1	a_2	af	S.F.	a_1	a_2	Q.F.	a_1	a_2	af	S.F.	a_1	a_2	Q.F.	a_1	a_2	af	S.F.	a_1	a_2
First quarter	26	42	4	4	37	87	89	11	64	19	9	31	90	74	35	20			34	91	71	46	2			16	56	44
Mid. half	48	48	12	10	71	86	60	42	62	7	7	82	72	68	62	34	9	3	56	70	59	48	31	2	1	56	71	61
Fourth quarter	27	19	4	4	41	90	80	18	61			36	86	58	42	17	5	2	36	75	53	49	33	6	6	16	69	87
Totals	101	39	8	7	149	87	76	71	62	7	6	149	79	70	139	25	6	2	126	77	60	180	24	3	2	88	68	62

a_1 = No. of associative aids, first day.

a_2 = No. of asociative aids, second day.

af = No. of associative aids working falsely on second day.

Under Q.F. and S.F. are stated the number of pairs in each quarter or half.

TABLE IV

	Q.F	S.F.
General average L.T. first day	12.41	7.63
General average L.T. second day	8.34	2.31
Per cent. saved	32	69
Number of subjects	27	27
Number of pairs	491	512
Per cent. of pairs with A_1	33	79
Per cent. of pairs with A_2	5	67
Per cent. of pairs with Af	4	0

with associative aids on the first day. The per cent. varies from 39 to 62 for the Q.F. pairs and from 68 to 87 for the S.F. pairs. Second, we notice that on the second day most of the associative aids for the Q.F. pairs are forgotten and nearly all of those that are not forgotten produce a false response; but most of the associative aids for the S.F. pairs are remembered. The per cent. of the associative pairs for the Q.F. pairs remembered varies from 3 to 8, and from 2 to 7 per cent. of them produce false responses. But the per cent. of associative aids for the S.F. varies from 60 to 76. The meaning of these facts is quite clear, but it may be made still more clear by a study of the general averages, totals, and per cents. in Table IV.

In Table IV. I wish to draw attention to the much smaller L.T.'s for the S.F. pairs, then to their much greater retention, then to the approximately equal number of Q.F. and S.F. pairs, then to the fact that more than twice as many of the S.F. pairs are learned with associative aids on the first day and more than 13 times as many of them are learned on the second day, and finally to the fact that four fifths of the associative aids for the Q.F. pairs remembered on the second day produce false responses. I believe that these facts establish a causal relation between the rate of learning and forgetting on the one hand, and associative aids on the other hand. This is confirmed positively by the high frequency of the latter in the S.F. pairs, and negatively by their low frequency in the Q.F. pairs. We also see that although the Q.F. and S.F. pairs are learned in qualitatively different ways, yet this qualitative difference is stateable in quantitative terms in the number of associations.

Confirmation of the above conclusions as well as suggestive evidence of why there is not a 100 per cent. frequency of associative aids in the S.F. pairs is obtained by calculating the number of promptings, R., required to learn the Q.F. and S.F. pairs as this is influenced by the presence and absence of associative aids. The results are given in Table V. From this we see that the presence of associative aids

TABLE V

	S.B.	R.C.	W.S.	W.P.
Ave. No. of R. for Q.F. pairs with associations........	2.14	1.78	2.58	3.56
Ave. No. of R. for Q.F. pairs without associations.....	3.15	2.48	3.57	4.92
Ave. No. of R. for S.F. pairs with associations........	1.51	1.44	1.65	2.93
Ave. No. of R. for S.F. pairs without associations.....	2.42	1.92	2.84	4.66

makes a considerable reduction of the number of prompting in both the Q.F. and the S.F. pairs, that is, they make learning easier. The greater number of promptings for the S.F. pairs without association suggest that high retention may be due to strong imprinting as well as the presence of an association. Because of this factor we would not expect a 100 per cent. frequency of associative aids in the S.F. pairs or 100 per cent. absence in the Q.F. pairs.

Having stated the general principles about the relation of associative aids to the rate of learning and of forgetting, it may be well to reproduce a few of the original records as illustrations both of the rules and of the possibility of treating the data statistically.

Miss Bar. 3:15 P.M. '15.	March 5	March 6
	6. clean 1″	
	5. No 7″	
	4. space 5″	
	3. No 7″	
	2. simper 3″	2. clean 1.5″
Mistake clean	1.	1. No 5″
First day	No. assoc.	
Second day	No. assoc.	
	2. stencil 4″	
Kimono stencil	1.	1. stencil 2″
First day	Thought of design stenciled on kimono.	
Second day	Thought of design stenciled on kimono.	

Miss Hill. 5 P.M.	May 4	May 5
	2. Tomato 1″	
Cheese tomato	1.	1. tomato 1.1″
First day	Both used in rarebit	
Second day	Both used in rarebit	
		3. blast 1.0
	2. blast 2.6″	2. No 3.6
Troll blast	1.	1. betide 1.7
First day	No assoc.	
Second day	No assoc.	
Miss Bar. 3:15 '15.	March 5	March 6.
	4. attack 4″	4. attack 3″
	3. No 5″	3. No 5″
	2. No 6.5″	2. No. 5″
Angriff attack	1.	1. No 1″
First day	No assoc.	
Second day	No assoc.	
	2. lemon 2.5″	
Citrone lemon	1.	1. lemon 2″
First day	Thought of something sour and a response to a three-syllable word.	
Second day	Same as yesterday.	
Miss Boss. 4 P.M., '15	May 25	May 26
		3. gom 6″
	2. gom 2.2″	2. niff 5″
Vif gom	1.	1. lif 5″
First day.............	Thought of folks chewing gum and coming down with wif.	
Second day...........	Nothing before first two promptings and then I thought of some one's mouth coming down with a vif in chewing gum.	
	4. ruc 7.4″	
	3. No. 6″	
	2. jog 4.”	
Zis ruc	1.	1. ruc 4″
First day	Thought of seeing the rock.	
Second day	Thought of seeing the rock.	

The above examples give the number of promptings, responses, and L.T.'s, and associative aids, if any, for the pairs and subjects named for the first and second days. The first prompting, 1, opposite the pair was the reading of the pair by the experimenter and had no response. Above it are the results of the second, third, and fourth prompting, as the case may be. Under the line containing the word-pair are described the associations if any for that pair, and

the date of the same. The individual records are of course not as regular as the averages but nevertheless show the rule.

It will now be in place to discuss the experiments of other investigators whose results bear upon the conclusions stated above. The first experimental results bearing upon the relation between the rate of learning and of forgetting were obtained by Müller and Schumann. They are given by Ebbinghaus, 'Grundriss,' I., p. 682, and are directly in agreement with the results of Table I., showing that the number of repetitions in relearning and the per cent. S. vary directly with the number of repetitions in learning. The conclusion drawn was that slow learners forget slowly. But we have seen that this depends altogether upon how we measure retention.

Following Müller and Schumann, Meumann made some experiments which he says justify the same rule. He does not give his data, but explains the relationship in terms of the adaptability of the learner's attention, his purpose or adjustment, his imagery and method of learning. The slow learner has fixed attention, steady purpose, visual imagery, and makes little use of secondary characteristics in the impressions. The quick learner has a highly adaptable attention, temporary purposes, auditory imagery, and makes use of secondary means, such as "special auditory and visual remembrances derived from non-essential circumstances which attend the learning."[1] He states that the more extensively we use the latter, the more transient is attention. This is just the opposite of our experimental results, and since Meumann does not give the facts for his statement, I shall make no further answer. I have made no extended study of the type of imagery upon learning, but some of my most rapid learners were so-called visual learners and some of my slowest learners were so-called auditory learners. The associations of the former were such as: "I saw the words" or "I saw the objects." The associations of the latter were such as: "I thought of corporal for 'Körper-body.'" On the other hand I have evidence which apparently confirms Meumann's state-

[1] 'Psychology of Learning,' tr. by Baird, p. 177.

ments on the type of imagery. My own observations lead me to believe that the rate of learning depends upon something much more fundamental than the type of imagery, namely, inherited ability and association. I should agree that the rapid learner has a more adaptable attention than the slow learner, but it is not usually concomitant with rapid forgetting.

Balaban[1] obtained some interesting results upon the influence of association upon the rate of learning and of forgetting. His subjects learned series of 20 unrelated word-pairs. They were instructed to learn one half of them logically and the other half mechanically. To test the influence of these words upon learning, the series were presented once and then tested by the Treffer Method. The logically learned series gave an average of 33.9 successes, and the mechanically learned series an average of 7.9 successes. To test the influence upon retention, the series were completely learned and tested after intervals of 3 and 6 days. Subject I. learned the logical series in 3 repetitions and had 3.3 successes. Subject II. gave similar results. Although the associations in this experiment were largely voluntary, whereas in mine they were largely involuntary and spontaneous, the results of both lead to the same conclusions.

Busemann[2] made groups tests on school children by the Treffer Method by reading to them four series having eight pairs of words from each of the following: concrete nouns, abstract nouns, adjectives, verbs, and meaningless words. The per cent. of successes for each of these were 80.5, 70, 51.7, 49.2, and 14, respectively. For 6 subjects who followed the method of complete learning, the results were:

	Nouns	Adjectives	Syllables
Saving method repetitions to learn	8.5	11.0	13
Per cent. S.	64	57	61
Treffer method R. to learn	10.5	15.1	22
Successes	65	55	41

[1] *Zeit. f. Psychol.*, 56, 1910, 356 ff.

[2] *Zeit. f. Angew. Psychol.*, V, 1911, 211 ff.

Busemann investigated the frequency of associative aids in one subject and found the following: nouns 88 per cent.; adjectives 57 per cent.; and syllables 54 per cent. It will be seen that this experiment in every way confirms the results and conclusions of the present experiment.

G. E. Müller[1] made a study of associative aids in connection with the remarkable memory of Dr. Rückle, who had a memory-span for 72 numbers, and found that he made use of many ingenious aids that would not occur to an ordinary person. For example:

841	suggested	29^2,
295	"	5 times 59,
635	"	5 " 127,

and that 127 is characteristic since it is the first prime number after 113. 535 suggested .535, the side of a circumscribed polygon. 548519 suggested that the difference between the first and the second halves is the prime number 29. 70128 suggested 701 plus 28 = 729 = 9^3. 901 suggested the year in which the Queen of Saxony came to the throne. Müller, after a careful study of these associations, concludes that he is justified in setting up as a general law that exceptional memory performances of this sort are conditioned by the coöperation of natural helps in so far as they do not rest upon mnemonic devices (p. 236). After reviewing the results of the experiments of Balaban, Busemann, Michotte and Ransy, and Ephrussi bearing upon associative aids, he notices that they shorten the learning time, heighten retention, and shorten the reaction time, and concludes that they have the advantage of strengthening the connections between members and of facilitating the transitions between complexes. On the other hand, they have the disadvantage of sometimes diverting the attention from the associated members and of leading to a reproduction of the aid instead of the associated members.[2] So far as my experiment goes, I have no grounds for dissent from G. E. Müller. If spontaneous verbal associations lower the learning time and heighten the retention of

[1] *Zeit. f. Psychol. Erg-Bd.*, 5, 1911.

[2] *Zeit. f. Psychol. Erg-Bd.*, 8, 1913.

verbal material, it would be the same psychological result if mathematical association of Rückle's type should increase the memory-span for numbers, facilitate rapid calculation, and produce high retention for numbers.

Among American investigators who made experiments, the results of which bear upon the relation between the rate of learning and of forgetting, are Whitehead, Henderson, Ogden, Pyle, Norsworthy, Woodworth, and Lyon. Whitehead studied the effect of visual and auditory modes of presentation upon learning and retention, but with regard to the rate of learning he concludes that the slow learner relearns in a shorter time and retains a larger amount than the fast learner. But Pyle, who recalculated his data after eliminating the results of one S. as erroneous, finds by adding the L.T.'s of the fast six and of the slow six that the six who learned in the shortest time also relearned in the shortest time.[1] Henderson, who had subjects read twice a selection from Irving's 'Dutch Homestead' and then had them write down what they could remember after zero, two-day, and four-week intervals, found that the quick learner recalled greater percentages of what they had learned than the slow learners. Ogden,[2] who studied the influence of the rate of reading upon learning and retention, found that the fast learners usually require less time for relearning than the slow ones, and that this is true both for meaningful and nonsense material. Pyle,[3] who had college students memorize 40 items from a book on nature study and relearn them after 24 hours, found that the quickest of four subjects learned the material in 4.1 repetitions and reproduced 38.5 ideas; and that the slowest required 4.7 repetitions and reproduced 37.5 ideas. Two other groups of four subjects each yielded similar results. From 6 subjects who learned series of 25 nonsense syllables forward by the method of daily repeated learning until permanent retention was obtained, and then learned them backwards by the same method, he found that the total number of repetitions required for permanent reten-

[1] Summarized from Lyon, *Arch. of Psychol.*, 1916, No. 34.

[2] *Arch. f. d. ges. Psychol.*, 1904, **2**, 93 ff.

[3] *J. of Educ. Psychol.*, 1913, **2**, 310 ff.

tion in each case was in proportion to the number of repetitions required to learn the series on the first day. According to Lyon,[1] Gamble, who read series of letters, words, and figures to 350 college students and then tested retention by the amounts which they could reproduce after one, two, and five(?) readings, respectively, found a marked correlation between facility in learning and relearning. In her recently reported study[2] of the effect of the rate of repetition on learning and retention, she finds no proof that rapidly repeated series are forgotten more rapidly than slowly repeated series. Her materials consisted entirely of nonsense syllables. Norsworthy[3], who had 83 college students study a list of 1,200 German-English vocabularies 20 minutes a day, 5 days a week, for three weeks and tested retention with selected lists of 50 German words at intervals of 2 days and of 30 days after the third week of study, found that the fastest half of the learners remembered 70 per cent. of the words in the first test and 73 per cent. in the second test. The slowest half remembered only 52 per cent. in the first test and 47 per cent. in the second test. Those who learned over 900 words within the limits of time for study remembered 76 per cent. in the first test and 78 per cent. in the second test, and those learning less than 300 words within the same time remembered 46 per cent. in the first test and 36 per cent. in the second test. The most extensive study upon the problem in question was made by Lyon,[4] who obtained results from 426 subjects, all being adults except 40. His materials for the most part consisted of series of 20 digits, series of 20 nonsense syllables, a series of 20 unrelated 3-letter words, a passage of prose having 100 words, and a passage of poetry having four 4-line stanzas. All of the materials were completely learned, and retention was tested after various intervals first by the amount which the subject could reproduce without a repetition, Method 1, second by the amount which the subject could reproduce after one repetition,

[1] *Op. cit.*, p. 10.

[2] Psychol. Monog., 1916, No. 96, p. 99 ff.

[3] *J. of Educ. Psychol.*, 1913, 4, 61 ff.

[4] *Op. cit.*

Method 2, and third by the Saving Method, for which the material was completely learned immediately after Method 2 was finished. Since his material may be roughly divided into meaningful (prose and poetry) and meaningless (all the rest), I shall give sample results of each kind from Table XI. for 24 normal college seniors:

NONSENSE SYLLABLES

	L.T.	Rl.T.	Per Cent. S.	Meth. 1	Meth. 2
Upper half	5.5′	6.5′	71	26	44
Lower half	15.0′	12.1′	68	21	36

PROSE

	L.T.	Rl.T.	Per Cent. S.	Meth. 1	Meth. 2
Upper half	14.3′	4.8′	68	59	80
Lower half	27.3′	9.1′	66	46	67

AVERAGE FOR ALL MATERIAL

	L.T.	Rl.T.	Per Cent. S.	Meth. 1	Meth. 2
Upper half	12.2′	4.3′	61	39	55
Lower half	21.9′	7.2′	66	30	46

Interval between L.T. and Rl.T. is one week for meaningless material and ten weeks for meaningful. From these and other results, Lyon concludes that "the most general statement that can be made is that those who learn quickly remember longest if the material is logical in character. Where the material is 'illogical' and is meaningful by 'motor associations,' so to speak, the converse is true. This, however, has many exceptions, depending upon the method used" (p. 58). From his study of the merits and demerits of the different methods of measuring retention, he concludes: "The different methods give opposite results, and yet in one sense of the word, one method is as 'correct' as another" (p. 56).

Whatever the value of the different methods used by Lyon may be, it is clear from the above results that the quick learners relearn more quickly and reproduce absolutely greater amounts than the slow learners. If a quick learner relearns his lesson more quickly and reproduces more of it than a slow one, it makes little difference from the standpoint of efficiency what the Saving Method tells us about his 'retention.' I ask again, how can a slow learner remember more

than a quick one when he reproduces less, or how can he forget more slowly when he has a longer relearning time? The lengths of the relearning times as well as of the corresponding results of Methods 1 and 2 clearly show the invalidity of the Saving Method as a measure of forgetting. If we accept this, then we must admit that Lyon's conclusions are at variance with his facts which really support the conclusion that I have drawn from my own data. Lyon's statement of his data, however, is similar to the one that I have used in Table I. We have seen that that method is not well adapted for solving the problem in question. If Lyon could have separated the associatively learned materials from the mechanically learned materials, he would have had little difficulty in solving his problem. In this connection, we notice his conclusion regarding logical material which is quickly learned and slowly forgotten. Is this because of the character of the material or is it because the prose is usually learned by a different psychological process than are nonsense syllables? We noticed from our results that the character of the material is of little importance in determining the relationship between the rates of learning and forgetting in comparison to the way in which that material is learned psychologically. Meaningful material differs from meaningless only in having a greater number of associative aids, and it is those which make possible Lyon's conclusions regarding logical material.

From this review of the facts of the American investigators, only one conclusion is possible, namely, that quick learning means quick relearning. To my mind this is the same as saying that quick learning means slow forgetting. But wherefore this relationship? It appears that none of the above named investigators considered this question, nor was its answer made possible by their methods which lacked the qualitative side. We have seen, however, that the study of associative aids has furnished a reliable solution.

Summary I.

The relation of the rate of learning to the rate of forgetting depends upon three conditions: (1) the character of the

measure; (2) the character of the learning; and (3) the character of the material. If the Saving Method is the measure of retention, then "Quickly learned, quickly forgotten" expresses the relation. If the amount reproduced per unit of time is the measure, then "Quickly learned, slowly forgotten" is true. The Saving Method is, however, a false measure of retention, for it does not express the efficiency of memory. This leaves only the second statement as true, which may be more accurately expressed as follows: the rate of relearning varies directly as the rate of learning, or the rate of forgetting varies inversely as the rate of learning.

The cause of this relationship is found in the character of the learning as shown in the influence of associative aids. Word pairs are quickly learned and slowly forgotten by means of associative aids. The absence of these associates produces slow learning and quick forgetting.

The influence of the character of the material may be stated as follows: For quickly forgotten pairs, the more meaningful the material, the more rapid the forgetting. For slowly forgotten pairs, the more meaningful the material the slower the forgetting.

A review of the literature upon the rate of learning and of forgetting shows that the facts of other investigators support the conclusion stated above, but they drew opposite conclusions because of an uncritical interpretation of the measure of retention.

II

If associative aids are of such great importance in determining the rate of learning and of forgetting, it will be a matter of interest to study them analytically and find their relation to other associations. Analytic studies of associative aids have been made by Ephrussi, Balaban, Busemann, and G. E. Müller. Since such a study and the resulting classification are matters of interpretation rather than of fact, I shall give no account of the classifications made by these investigators. I have largely followed the demands of my materials, and, so far as possible, have adopted the usual categories for this purpose.

Since the material of the experiment consisted of paired associates, it is possible that such an association should be connected with (1) both words, the stimulus and the response, or (2) with the first word only, the stimulus, or (3) with the second word only, the response. The associative aids were accordingly distributed with reference to these situations. The next step in the classification was to distribute the associative aids of each class with reference to their most outstanding relationship to each situation. In naming this relationship, no account was taken of the learner's opinion, nor was such an opinion asked. The subject having reported the association, it was treated as so much matter to be classified with respect to such relationship as the experimenter could find between it and the situations with which it was connected. These relationships, therefore, do not tell us what psychological factors produce these associations, but only give us some ground for a further inference as to what these factors might be, and on the theory of probability help us to predict their recurrence in the future under similar circumstances. Since a classification of associations with respect to two words is rather unusual, the following table of categories will give examples:

Associations of			
Similarity in sound or sight of	Letters	Zis ruc	'Z—R'
	Syllables	Mistake clean	'Take lean'
	Rhythm	Heb tup	'Head top'
Similarity in meaning of	Contrast	Chest muffin	'The one is large and the other is small.'
	Verbal Habit	Körper body	'Corpus body'
	Coördination	Celery wafer	'Lettuce and crackers.'
	Predication	Simmer tarry	'Both slow.'
	Adjectival relation	Mistake clean	'Clean mistake.'
Thoughts of patterns like	Words	Simmer tarry	'Cemetery.'
	Objects	Ring kitten	'A ring around a kitten's neck.'
Thoughts of	Words only	Troll blast	'Saw the words.'
	Objects only	Ribbon banana	'A pink ribbon beside a banana.'
	Words and objects	Soup wall	'Saw the words and the objects.'
Contiguity	Order and position	Geigen fiddle	'Last pair.'
	Time	mif jas	'Third one I missed.'

Table VI

Connected with	Per Cent. of Associative Aids Consisting of															Total Number
	Familiar Words or Phrases Similar as to								Thoughts of					Associations of Contiguity in		
	Sound or Sight of			Meaning in					Patterns Like		Only Words	Only Objects	Both Words and Objects			
	Letters	Syllables	Rhyme	Contrast	Verbal Habit	Coördina-tion	Predica-tion	Adjectival Relation	Words	Objects				Order and Position	Time	
S.B.																371
Stimulus and response		.27	.81		.27	.81	11.34	2.70	.81	29.97	3.24	3.78		31.86	.27	348
Stimulus only		.27				.54		.81				1.62				12
Response only		.54				.27		.54		1.08		.54				11
R.C.																344
Stimulus and response			3.48		.29		26.97		.58	15.08	4.35	10.15	.58	29.58	3.19	327
Stimulus only						.29										1
Response only			.87		2.61					.87			.29			16
W.S.																367
Stimulus and response			16.32	1.63	8.97		4.35		7.88	3.53	4.08	1.90		43.24	2.44	331
Stimulus only			1.90				.27									8
Response only			.54				1.36	.27				5.44				28
W.P.																266
Stimulus and response			28.20						3.76		3.76			40.98	4.13	215
Stimulus only		.37	2.63													8
Response only	1.88	.75	11.28						2.25							43

Table VI. gives the percentile frequencies of the associative aids distributed with respect to the three classes named above and for each series used in the experiment. Some of the outstanding features in this table are: (1) Nearly all of the associative aids are connected with both the stimulus and the response, *i. e.*, with both words of a pair. (2) The category of 'order and position' has the highest frequencies. (3) The second highest frequencies are in the associations of predication and of patterns for English pairs, and in the associations of rhythm for German-English vocabularies and for nonsense syllables pairs. (4) If we except the associations of contiguity, we can say that associative aids connected with words of the mother tongue cluster about some likeness in meaning while those that are connected with some foreign language cluster about some likeness in sensory quality. (5) With respect to associations of order and position we can say that these are less frequent with familiar material than with strange material.

If from these facts we attempt to infer the influence of the quality of an association upon its function, it seems that logical associations reduce the rate of learning much more than sensory ones, and are therefore much more important for this function. The evidence for this statement is proposition No. 3 above and the fact that the one L.T. of the first day for the familiar pairs is 6.29″ and for the strange pairs it is 14.69″. We have seen that associative aids of all kinds are favorable to learning. We now see that the degree of their favorableness varies with their quality, the logical being much more favorable than the sensory.

A word of explanation should be added for the high frequencies for the associations of order and position. The series were learned always in the same order from day to day, and this condition no doubt accounts for their high frequencies.

The next question concerns the relation of associative aids to other associative and reproductive tendencies. By the latter I mean those that interfere with the learning process and cause a wrong response. Since associative aids are also

a source of error, it is possible to determine the frequency of error due to this source as compared with that due to other sources, and therefore the comparative value of these associative tendencies for learning and retention. In analyzing these errors, I have followed the same general plan as for the associative aids, but had to make some omissions and additions of the categories. I omitted the response immediately connected with stimulus only and with response only and have added 'mediate with stimulus,' 'mediate with response,' and 'perseveration.' The mediate associations were frequent enough to demand this, and since they were rather unusual, I shall reproduce some specimens:

Mediate through stimulus by similarity in sound or sight of
- Letters:
  - Wip*f*el-summit; Angri*ff*-attack.
- Syllables:
  - *Be*harren-persevere; *Be*sonder-particular.
  - *An*regen-stimulate; *An*griff-attack.
- Rhythm:
  - Er*eig*nen-happen; G*eig*en-fiddle.
  - G*eig*en-fiddle; Er*eig*nen-happen.
- Verbal habit:
  - Beq-vog; Beckon-nod.

Mediate through response by similarity in sound or sight of
- Letters:
  - Yab-*l*ek-*l*uck.
- Syllables:
  - Ereignen-*hap*pen-*hap*py.
- Rhythm:
  - Dev-*cux-coax*.
  - Paf-*zal-jal*.
- Verbal habit:
  - Pork-*cocoa-bean*.
  - Ring-*kitten-cat*.

In the above examples, the last word is the response given and the first word is the stimulus. The elements in the stimulus or other word printed in italics are the factors which in the judgment of the experimenter might have led to the response named. For example, Ereignen was often responded to by fiddle, probably because the sound of *eig* occurs both in Ereignen and Geigen. There is no proof that ereignen suggested geigen, and that geigen then suggested fiddle. It is more probable that both happen and fiddle during the

early stages of learning were associated with the sound of *eig*, and sometimes it suggested happen and sometimes fiddle. But this type of association I have called mediate because a certain stimulus suggested a second response whose stimulus had something in common with the first stimulus.

Besides mediate associations, I have added perseveration as a category for the distribution of error because many of the responses could not be explained otherwise. The criteria of perseveration were: (1) repetition of a response on the same day after it had been given at its correct place, (2) repetition of a response stimulus after stimulus because the subject could apparently think of no other and knew that it belonged somewhere in the series, (3) when a response was given to a stimulus with which it had no discoverable associative connection.

The criteria of associations of order and position should also be stated at this point. When two or more responses were given in the correct order but out of position, or when a word or syllable was named one position in advance of its proper place, the errors were classified as due to associations of order and position.

TABLE VII

	Per Cent. of Wrong Responses Due to Association																	Perseveration	Total Number of Wrong Responses
	Immediate with Stimulus and Response by								Mediate with Stimulus by Similarity in Sound or Sight				Mediate with Response by Similarity in Sound or Sight					
	Associative Aids	Similarity in						Contiguity										
		Sound or Sight			Meaning													
		Letters	Syllables	Rhyme	Verbal Habit	Coördination	Adjectival Relation	Predication	Order and Position	Letters	Syllables	Rhyme	Verbal Habit	Letters	Syllables	Rhyme	Verbal Habit		
S.B.....	14.90			0.97	1.62			0.32	35.96				.97	.97	.32	5.83	3.24	34.09	305
R.C.....	5.55			.46	4.16			1.39	42.59							2.77	7.40	35.18	216
W.S.....	3.19		1.66	.49	1.82				28.05	1.99	11.28	2.49	0.16	2.15	.66	2.32	.16	43.49	602
W.P....	3.38	.32		7.72					26.56			1.12	1.28	4.83		30.59	.80	29.78	621

Table VII. gives the results of the distribution of the errors according to their type of association. The noteworthy features of this table are that an average of 35.29 per cent., or little over one third of the errors are due to perseveration. An average of 33.29 per cent. or one third

are due to associations of order and position. The other third of the errors are distributed chiefly among the associative aids and mediate associations. Only 6.78 per cent., or one fifteenth of the errors, are due to associative aids. In all of these cases, the aid was reproduced instead of the associate. There are almost no errors due to logical associations. It therefore appears that the small per cent. of error due to associative aids and the absence of error due to logical associations are negative confirmations of the importance we have attributed to them for learning and retention. While associations of contiguity are important for learning, they produce many errors before becoming fixed. The most important source of error is due to perseveration. That over one third of the errors must be attributed to some such factor as perseveration may be taken as strong proof for its existence and as a confirmation of the evidence of Müller and Pilzeker who named it. Interference from perseveration occurs chiefly during the early stages of learning while the various associations are in process of formation. After associations are once formed and the series learned, their chances of interference with second series are greatly reduced not only because of their fixed character but also because the learning of the second series is directed by a new and different adjustment or Aufgabe. The present experiment also shows evidence of this, for the four series of associates were learned in immediate succession but I can find no errors or responses that were carried over from one series to the next, and on this basis there is no evidence of perseveration. This, however, does not contradict the role which it plays within the series, and I cannot, therefore, accept De Camp's[1] evidence as proof. It should be emphasized that perseveration according to the evidence of this experiment is an inhibitive and disintegrating factor in learning. It produces interference and error and allows their ideas to flow away from their anchors. It is so far uneconomical and destroys the adaptability of the mind while the latter is attempting to do its proper work. It, therefore, appears as a symptom of weakness and inefficiency.

[1] Psychol. Monog., 1915, No. 84.

But it must be remembered that our experiment furnished no measure of whatever positive benefits it might have.

If we examine the facts of Tables V. and VI. with respect to the frequency of the various types as compared with those for free association, we notice many differences. For example, in Table V. there are no categories for superordination, subordination, cause-effect, verbs, participles, verb-object, word-compounding, etc., such as those used by Woodrow and Lowell. All of these excepting verbs, participles, and word-compounding were in my original table, but none of these types occurred. Those that did occur are distributed very much differently from those in free association, the striking differences being the high frequencies for order and position, patterns, predication, and rhythm. In Table VI. the distribution is still more concentrated, there being only three general types, perseveration, contiguity, and similarity in sensory quality. This is probably due to the fact that associative aids were spontaneous but the responses for the facts of Table VI., excepting the errors due to associative aids, were controlled, *i. e.*, they were selected from the material given and were only misplaced. In a measure, the associative aids were also controlled. They were directed by the Aufgabe to learn the pairs given and had to be adapted to these materials. The Aufgabe and the nature of the materials thus require no correspondence of frequency in types with those of free association.

Summary II.

An attempt to classify associative aids under the usual categories for free association shows a much more concentrated distribution with respect to the number of classes, the principal classes being order and position, patterns, predication, and rhythm.

Their relation to other associations as indicated by an analysis of errors shows that only about 7% of the errors in learning are due to them, leaving 93% due to other reproductive tendencies the most of which may be classified under perseveration, contiguity and sensory similarity.

The distribution of the types of associative aids and of the errors in learning has little agreement with that for free association because the former are a form of controlled association.

So far as the function of associative aids is correlated with their type, it appears that those based on meaning are much more effective in learning and retention than those based on sensory quality, but all types have a favorable influence on mental economy. As such they are distinguished from perseveration, which is a disintegrating factor in the work of the mind.

SIMULTANEOUS VERSUS SUCCESSIVE ASSOCIATION[1]

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In a recent number of the *British Journal of Psychology* A. Wohlgemuth[2] attempts on the basis of certain experimental results to decide between the rival theories of simultaneity and succession as conditions of association. He argues that, if the former is the true theory, simultaneous presentation should be more favorable for association than successive. If the latter theory is true the reverse should be the case. The results which he obtains are regarded as favorable to simultaneous presentation and the conclusion drawn that association takes place only between experiences which occur simultaneously. Apparent cases of successive association are to be explained as due to simultaneity between the succeeding experience and the disappearance, or 'akoluthic,' phase of the preceding one.

Objection may be made, however, both to the method of the experiments and to the interpretation of the results. In the first set of experiments, in which four observers were used, all give more favorable results with successive presentation, though the variability of the averages, and hence the reliability, is not given. But these results are considered irrelevant and discarded because the exposure time was not the same in the two series. Each member of the pair in the successive series was exposed for the same length of time as the pair of simultaneous stimuli, and hence it was held that the 'learning time' was twice as long in the successive as in the simultaneous presentation. To equalize the time two different methods were used. In one set the exposure time for the simultaneous pair was made twice as long as for each

[1] From the Psychological Laboratory of the University of Michigan.

[2] A. Wohlgemuth, Simultaneous and Successive Association, *Brit. J. Psychol.*, Vol. VII., 434 ff.

member of the successive pair, the number of presentations remaining the same in the two series. In the other set the exposure times were the same as in the original experiments but the number of presentations was twice as great in the simultaneous series as in the successive. The last method must be objected to because it disregards the effect of distributing the repetitions. The first implies that the strength of an association depends upon the exposure time of the stimuli only, regardless of whether or not they are exposed at the same time, and thus contradicts the theory which was to be proved, namely that association takes place only while the two experiences are present together in consciousness. Both methods are, therefore, based upon inadmissible assumptions and must be rejected. But even if the assumptions were admitted, no safe conclusion could be drawn from the results, because the differences in favor of simultaneity are in every case less than the P. E. and in five cases out of the sixteen they are negative. As far as determining whether simultaneity or succession is the real condition of association these experiments prove nothing.

The present investigation is an attempt to repeat Wohlgemuth's experiments with the objectionable features omitted. It seemed also desirable to determine, apart from any theory concerning the conditions of association, the relative merits of the two methods of presentation as well as the effect of increasing the interval between the two stimuli in successive presentation.

The material consisted of seven sets of seven series each of nonsense syllables, each series consisting of five pairs. One set was used for simultaneous exposure, the other six for successive, varying from immediate succession to succession after an interval of five seconds. It was decided to have seven series of each kind of exposure in order that each one might successively occupy the positions one to seven in the daily program and thus neutralize any possible effect of practice or fatigue. For this purpose 245 pairs or 490 different stimuli were needed, and nonsense syllables, though not free from objections, seemed the only material available.

Since Wohlgemuth used only 24 pairs or 48 stimuli, he was quite justified in using other kinds of material. The exposure apparatus consisted of a chronograph in which the pen was replaced by a movable screen with a suitable aperture. The syllables were pasted on a sheet of paper, which in turn was fastened to the drum in such a manner that the rotation of the drum would expose them either simultaneously or successively at desired intervals, and also present the first members of the pairs for a test series. In the latter, the syllables were never presented in the same order as in the learning series, in order to eliminate position as a factor in association. The exposure time varied from ⅓ to ¼ second, but was kept constant for each observer. The interval between the pairs was always 6 sec., between exposure and test 10 sec., and between successive series 12 sec. The arrangement also permitted a variation in the number of exposures of each pair, a condition made necessary by the difference in learning ability of the observers. In most cases only two repetitions were necessary, but in one case three and in another four were used.

The observers, all students in psychology and the majority undergraduates, were instructed to adopt a purely passive attitude, to observe attentively but make no particular effort to memorize the syllables, to refrain from pronouncing them either orally or mentally, to make no special effort to retain them, and above all to refrain from all sense associations. At first it was found rather difficult to follow these instructions, especially with regard to internal speech, but after a few trials these difficulties seemed to have disappeared. Even the tendency to articulate the syllables disappeared except in the simultaneous series, where it persisted to the end. This tendency took the form of combining the two syllables into one word and occasionally reading meaning into it. For example, the two syllables 'dis' and 'mel' appearing together were read as 'dismal.' Whenever the connection of the syllables with an idea was strong enough to be considered a help in recalling the associate the result was not included in the average for that series. Most of the observers had

little practice in introspection and for this reason introspective accounts were asked for mainly for the purpose of seeing how faithfully the instructions were carried out. The introspections are in essential agreement on three points: (1) that simultaneous exposure taxes the attention more than successive, (2) that there is a persistent tendency towards articulation in the simultaneous series, and (3) that when in successive association there was an appreciable interval between the stimuli, the first would remain in consciousness, though with varying degrees of intensity, until the second arrived.

The results from seven observers are presented in the following table in terms of per cent. of right responses, including averages, average deviations (δ), differences between averages, and the P. E. of the differences.

TABLE I.

	Simultaneous		Successive											
			0		1		2		3		4		5	
	Av.	δ	Av.	δ	Av.	δ	Av.	δ	Av.	δ	Av.	δ	Av.	δ
Bu.	67	20	56	22	44	16	53	18	44	22	58	14	46	32
D.	67	18	51	18	53	26	66	14	62	10	46	22	59	20
G.	69	22	47	16	52	10	38	10	38	14	28	16	34	18
I.	52	14	44	14	37	22	24	20	31	20	23	16	37	10
L.	34	18	23	12	33	18	38	16	40	22	43	16	41	6
M.	46	14	28	24	25	30	34	20	60	20	43	26	28	18
Br.	90	14	98	4	92	10	81	20	80	28	87	18	81	24
Av.	61	18	49	16	45	18	48	16	51	20	49	18	49	18
Diff.			12		4		−3		−3		2		0	
P. E.$_d$			7.7		7.7		7.7		8.2		8.6		8.5	

The difference between the simultaneous and successive series is small, less than twice the P. E. Nevertheless, since the simultaneous exposure yields better results in every case except one, the average difference of 12 per cent. in its favor seems significant. But the expected decrease with increase of the interval between the paired stimuli is not found. Association is made as readily between syllables five seconds apart as between those in immediate succession. The introspections must be resorted to in order to explain these results. These clearly show that the limit of duration of

the primary memory image had not been reached even in the case of the longest interval. One conclusion seems to follow from these results: the effectiveness of the memory image for association does not appreciably decrease for at least five seconds after the stimulus has ceased.

The slight superiority of simultaneous presentation does not necessarily prove the theory of simultaneity; it is more readily explained by the tendency to articulate and combine the simultaneous syllables into a single word.

These results, however, are not quite comparable with those of Wohlgemuth, for this investigator expressly rejects results based upon experiments with nonsense syllables and maintains that material free from the influence of motor speech habits alone give psychological association. Another group of experiments was, therefore, devised in which colors and letters of the alphabet replaced the nonsense syllables. Geometrical figures would have been preferable to the letters, but could not be used on account of the large number needed and the difficulty of making them distinct and yet small enough to fit the apparatus. Twenty-eight colors were used, sufficiently different to be clearly distinguishable. Besides the 26 letters of the alphabet the signs & and $ were used. This gave four series of seven pairs each, two of which were simultaneous and two successive. Twelve other series were obtained by arranging the colors and letters in different combinations. Each pair was exposed twice. The exposure time was ⅓ sec., as before. Suitable arrangements to eliminate the effect of practice and fatigue were made. Other details were the same as in the previous experiments.

The results are given in the following table in terms of per cent. of right responses, including averages, average deviations, differences, and the P. E. of the differences.

Here again the differences between the two methods of presentation are small. In only one case does the difference exceed twice the P. E. But every one of the eight observers gives better results with successive presentation, and it is unlikely that this is due to mere chance. It may, therefore, be assumed that the observers possess the same type of

associative mechanism and that it is permissible to pool the results of the group. When this is done an average difference of 6 per cent., with a P. E. of 1.9 per cent., is obtained in favor of successive presentation. Since the P. E. is less than ⅓ the difference, it is extremely unlikely that the result can be due to chance.

TABLE II.

	Simul.		Suc.		Diff.	
	Av.	δ	Av.	δ	Av.	$P.E._d$
B	58	18.0	65	20.0	7	5.3
D	71	11.2	75	14.2	4	1.8
G	35	16.0	36	14.6	1	1.8
I	39	20.6	47	24.0	8	6.7
L	37	24.8	44	9.6	7	5.5
M	51	20.4	59	16.0	9	5.3
Ge	25	14.6	27	12.4	2	4.3
K	25	16.6	33	16.4	8	4.8
Av.	42	17.8	48	15.9	6	4.2
Pooled results of all	....	....	....	....	6	1.9

It appears, then, that the results vary with the kind of material used. Nonsense syllables are more readily associated when presented simultaneously, colors and letters when presented successively. The suggestion immediately arises that it is purely a matter of attention in relation to the nature of the material. If the material is such that the pair of stimuli form an organic unit, or can be attended to as a unit, simultaneous presentation is preferable; when such that the members of the pair come as separate entities, or must be attended to separately in order to be apprehended, successive presentation is preferable. If a complex stimulus can be attended to as a whole, simultaneous presentation increases the bonds between its parts; when this is not possible, the part not attended to will be lost and fail to be associated with the rest. The physiological basis of association may be, and very likely is, the same in both cases.

In order to eliminate the persisting memory images during the interval between the two stimuli in successive presentation two-digit numbers were inserted between these. The observers were required to read the numbers aloud and keep their attention centered upon them rather than on the syllables,

except when the latter were exposed. It was planned to have the observers add some constant number to the numbers exposed in case the reading of them should prove insufficient to occupy their whole attention. But this was not found necessary. Every one of the observers reported that the reading of the numbers effectively and completely obliterated the memory image of the preceding syllable. It was thought that by this means a measure of the strength at various points of the neural 'akoluthic' phase could be obtained. In this part of the experiments each stimulus pair was exposed four times. In all other respects the material as well as the procedure was the same as in the first part of the experiments.

The results in terms of right responses are given in the following table.

Table III.

	Simultaneous		Successive												
			0		1		2		3		4		5	
	Av.	δ	Av.	δ	Av.	δ	Av.	δ	Av.	δ	Av.	δ	Av.	δ
O.	49	15.4	44	26.3	46	29.2	41	18.8	59	19.4	27	12.2	24	18.8
J.	33	14.0	31	12.6	17	13.4	14	13.4	20	12.2	9	12.0	18	16.0
H.	77	18.0	50	17.2	31	13.2	47	26.0	39	18.8	24	20.2	21	13.8
S.	59	16.6	56	23.2	42	11.2	42	11.8	42	25.4	34	20.8	24	20.8
Av.	54		45		34		36		40		24		22	
Diff.		9		11		−2		−4		16		2		
P. E.$_d$		6.8		5.5		5.6		6.4		6.2		5.5		

There is thus a distinct, though irregular, decrease in the number of right responses as the interval between the members of the pair in successive presentation increases. At the greatest interval used there are still 22 per cent. right responses. It may be argued that association in this case is due not to the waning neural activity set up by the preceding stimulus but to connection with the apparatus or the task. In such a case, however, any syllable of the series should have an equal chance of being recalled, and the chances for a right response should not exceed 20 per cent. If some of the syllables of the series could not be recalled, or, as sometimes happened, syllables from other series were recalled, the chances for a right response on the basis of association with the task alone must be still smaller. The maximum

strength of this factor, therefore, can not exceed 20 per cent., and is probably much less.

It appears, then, that simultaneity of two experiences is not necessary for an association to be formed between them. Nor is it necessary, though helpful, that memory images of the preceding experience be present simultaneously with the succeeding one. An association may still be formed between two experiences where the first has already passed out of consciousness when the second one appears, or in Wohlgemuth's terms, between one experience and the unconscious 'akoluthic' phase of another.

DISCUSSION

MISS CALKINS'S CASE OF SELF AGAINST SOUL

I.

On first reading Professor Calkins's article, 'The Case of Self against Soul' (THE PSYCHOLOGICAL REVIEW, July, 1917) I thought she had brought to light something in Plato that had escaped my notice. When she points out that Plato treats the soul sometimes as the principle of life and sometimes as the self, of course she is on familiar ground; but when she argues that he holds also a 'metaphysical' conception, she makes a point that I had never recognized.

In the end, however, she does not convince me. An indispensable feature of the 'metaphysical' conception of the soul as contrasted with the self conception appears to be that the soul is not conceived as, like the self, essentially conscious. To this she would doubtless assent, but she adduces no evidence that Plato ever wavered in his identification of soul with consciousness. What she does argue is that he treats the soul as immaterial, simple, unchanging; also, incidentally, as imprisoned in the body. Of course the proofs of these points are clear, but they do not show any conception distinguishable from the self conception. In fact, all the evidence appears to me to show that these characters are actually ascribed by Plato to the self.

For the soul is immaterial; but so is the self. The soul is also *ἀσύνθετος*, *i*, *e*., as Plato explains ('Phædo,' 78), not put together out of independently existing entities; but the self is likewise *ἀσύνθετος*,—Plato's proof of the simplicity of the soul in fact rests on its self-character. And the soul is changeless, *ἀεὶ κατὰ ταὐτὰ ὡσαύτως ἔχει* ('Phædo,' 79 *D*, etc.), when, acting according to her own proper nature, she contemplates the unchanging ('Phædo,' 79 *C*, *D*), though she can be dragged down into the region of the changeable; so would the self be changeless if it were contemplating only the unchanging, since there could be no possible occasion to change. In the passage just cited the argument for the changelessness of the soul is drawn from her self-character. As for her imprisonment in the body, Plato twice points out ('Cratylus,' 400 *C*,

'Phædo,' 62 *B*) that this is not a doctrine of his originating; when he gives his own interpretation of the 'imprisonment' ('Phaedo,' 65, 66) he puts it wholly in self terms. Thus all the characters that Miss Calkins adduces to show that Plato had a 'metaphysical' conception of the soul are characters ascribed by him (and by her, I should think) to the self.

If one does not attribute to Plato any separate 'metaphysical' conception, the other two conceptions coalesce, and his whole doctrine is harmonious as far as the nature of the soul is concerned. For life-giving character and consciousness appear to have been thought of as, strictly speaking, aspects or functions of one and the same soul. So Aristotle regards them ('De An.,' Bk. I., Ch. II., p. 403 *b*, 26–7), and so, apparently does Plato; life and *νοῦς* must be in soul ('Sophist,' 249 *A*)—not, however, as separate contents, else he would hardly argue directly from one to the other in the 'Laws' and in the 'Phædrus' myth. As I understand him, the two aspects are inseparable—the self *is* what vitalizes. In fact no special violence would be done to Plato if one were to translate *ψυχή* steadily as 'self.' Plants, I suppose, were not regarded by him as having a *ψυχή* that is not a self, for in the only places in which he touches on the matter, as far as I remember, he calls plants animals, or kindred to animals ('Timæus', 77, A, B, 80 *E*). It is presumably with Aristotle that the conception of a merely nutritive *ψυχή* originates.

Miss Calkins may of course have more or less in mind some other meaning for 'metaphysical' than appears in her article. As she points out, soul-substance as conceived, *e. g.*, by Locke appears to differ from self in that its existence is something over and above its experience. What that 'over and above' is, I regard as irrelevant in case of Plato, because I do not find in him any trace of a belief in it. I agree with her that, as a safeguard against such a conception, the abandonment of the term 'soul' would be of value, even though 'self' is not free from the danger of like misinterpretation. Ultimately I hope 'soul' may come back to its Aristotelian meaning. Hegel has convinced me that there is need of such a word; in spite of fantastic details his treatment of life (embodied soul) impresses me as on the whole one of his greatest successes.

MARY S. CASE

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II

In her article, 'The Case of Self against Soul' (published in The Psychological Review for July, 1917) Miss Calkins finds that one of the main sources of prejudice in regard to a 'self-psychology' is the traditional confusion of self with soul. By means of an historical study she has undertaken to distinguish these conceptions, hoping thereby to aid in removing the prejudice against the self as a legitimate standpoint in psychology. The study, as she says, "culminates in one insistent conclusion: the soul must go. As an historic concept of immense influence, it will always retain its prominent place in the history of ideas; as a term of modern psychology it has outlived any use it may once have had and has become a source of mischievous confusion."

Quite apart from the use which Miss Calkins has made of the results of her study of the classical systems in support of her own point of view, or at least in seeking to gain for it a fairer and more unprejudiced examination, this paper is noteworthy as recognizing that modern psychology can profit by an historical examination of its working conceptions—that, indeed, such investigations are necessary to enable it to avoid confusion and prejudice. It is noteworthy, also, as an example of careful and scholarly work, and the distinctions to which it leads are highly significant, both for psychology and philosophy. I agree with her in believing that in both fields there has been a good deal of confusion because of the identification of the concept of 'self' with the metaphysical (in a bad sense) notion of a 'soul' (or as I have been accustomed to say, a 'soul-substance'). This confusion, I agree in believing, has operated prejudicially in preventing oftentimes both a fair hearing and a clear understanding of the arguments for the concrete notion of a 'self.'

My main purpose, however, in commenting upon Miss Calkins's article lies apart from these considerations. While I agree with her in maintaining that, for some purposes at least, it is essential that psychology should be written in terms of the life of a 'self,'[1] it has always seemed to me that in defending this doctrine she has taken up an indefensible position in insisting that the 'self' is actually a 'perceived' fact, a particular datum of introspection. In the paper under discussion, this question does not come up for explicit consideration; but it is suggested that one explanation of the failure of psychologists to recognize the self is their 'inattention' to a content

[1] Cf. my article, 'The Standpoint of Psychology,' *Phil. Rev.*, 1914, 23, 159–166.

which is 'ubiquitous.' As an example of the tendency to overlook constant or ubiquitous contents, reference is made to the sensations of pressure from the clothing, followed by the statement, 'so in my introspection I simply forget to name myself' (pp. 278–9).

Now I am not sure that Miss Calkins would rest her whole case in regard to the self upon the possibility of discovering through introspection some specific self-content, parallel, in some sense, to the sensations arising from the pressure of clothing. If so, the basis appears to me very precarious. My own introspection would compel me to agree on this point with the exponents of 'psychology without a self.' If under the name of self we are looking for some 'psychological state,' Hume's famous saying that 'we can never catch the self' would seem to me to be the last word. But, apart from introspection, does not the concrete idea of the self forbid any claim to know it as an object? It appears to me that such a claim is intelligible only so long as one thinks of the self as a particular 'thing' or substance, analogous to other things, and differing from them, not in principle, but merely in degree of 'constancy,' or of 'ubiquity.'

But if the self is the self, *i. e.*, the universal subject, surely it cannot be known as an object without losing its character. And is it not true also that its concrete universality cannot be defined or adequately characterized in terms of ubiquity and constancy? What is essential, it would seem, in approaching a study of the mind from the point of view of the self is to adopt a new standpoint, to appreciate and describe in terms of a new category, rather than to discover some new specific content.

I am not sure that this is true of Miss Calkins, but I think that oftentimes the motive in insisting upon the self as an observed fact of observation, is to maintain its unambiguous reality, as something more than an inference, or a mere hypothesis. With that *motive* I entirely sympathize, but, already suggested, I think that it is mistaken in the contentions it employs to support its conclusion. For surely the reality of the self cannot be established by crying, 'lo here! or, lo there!' It is the universal subject of all experience, but cannot be turned into an object by psychology, and pointed at as *the thing that is 'highly complex,' 'persistent,' 'unique,' etc.* That is the soul-substance over again under a different name! But, as I think Miss Calkins would agree, a 'self-psychology' is not a psychology which requires us to find some specific content or object named the self, but it is a standpoint which sees the self as

the universal subject of all experience, as the universal category or speculative idea that gives unity and significance to the various elements in the mental life. The self is not substance but subject, not a mere particular 'existence,' but a true individual—something more and other than the type of reality which one can point at or 'hold in the hand.'

But how is the self known? If it is not an observed fact, is it a mere inference; and if so, what facts compel the inference? It seems to me that these questions are relevant only so long as one keeps thinking of the self as a particular fact for psychology, on the same plane as other facts. The reality of the self, one might say, is not a fact or an inference for psychology, but is the basis for all observation and inference in psychology and elsewhere. One cannot prove its reality without using it.

I think, however, that Miss Calkins means that the knowledge of itself which the self possesses is direct, and that in this sense it may be called a fact of observation, and not an inference. Undoubtedly the self knows itself—that is involved in being a self—but is this knowledge simply a 'fact of observation' or introspection? These terms are customarily employed in psychology to denote the direct apprehension of particular contents. When it is asserted that the self, too, is apprehended in this way, is there not danger that its real nature as universal subject shall be obscured, that it shall be regarded merely as a constant and ubiquitous element of consciousness, like the constant sensations arising from the pressure of clothing?

J. E. Creighton

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Reply

I shall try not to abuse the courtesy of the editor who permits me to reply to the comments of my friendly critics. I am impressed with the strength of the argument which leads Professor Case to identify Plato's 'soul' with 'self'—especially impressed with her analysis of 'Phædo,' 65, 66 and 79 *C*, *D*. But I question her assertion that as the soul is immaterial 'so is the self.' For the self is non-material rather than immaterial; immateriality is incidental not essential to it. And the considerations urged by Miss Case are not, in my opinion, entirely incompatible with the view that Plato wavers between, and does not identify, 'self' and 'conscious soul.' (For doubtless Plato's soul is conscious as well as immaterial and lifegiving. This is expressly stated in 'The Case of Self against

Soul.') But it is beside my present purpose to consider further the topic, in itself of great interest, of Plato's soul-doctrine and the misconceptions of it which Miss Case attributes to me. For, as she does not fail to point out, the main contention of my paper—that the self must be distinguished both from life and from a merely 'immaterial' soul—is unaffected by the failure or the success of my interpretation of Plato.

Professor Creighton does not, I think, fully realize the extent of my agreement with him. In his admission that we directly know the self as an 'unambiguous reality,' as 'more than an inference' and 'more than a hypothesis,' I find the essentials of my view. I believe with him that the self is not adequately defined in terms of ubiquity and constancy,' for I hold that, as *sui generis*, it is essentially indefinable though (incompletely) describable. And I further agree with Dr. Creighton that the self is not 'a perceived fact,' not a 'content,' not a 'psychological state.' It is not properly called a 'content' or a 'state' because both terms imply fragmentariness and momentariness. And my introspection, like Creighton's and Hume's, discovers no sensational or affective complex constituting a 'specific self-content.' (My comparison, it should be noted of common inattention to the self with inattention to 'sensations of pressure from the clothing' is in no sense intended as a complete analogy between the self and any group of sensations.) But though the self, as basal to all other psychic facts, certainly is not 'on the same plane' with them, I hold that it is none the less a fact in its own right and on its own deeper level. It is a fact because, as Dr. Creighton also believes, it is directly known, not merely inferred. And, as known, it is properly called an object.

Dr. Creighton makes only one positive statement about the self-of-psychology with which I find it difficult to agree. To call the self a 'universal subject of all experience' seems to me a metaphysical, not a psychological, assertion unless the words refer only to the individual experience, meaning simply: wherever there is consciousness a self is being conscious.

Mary Whiton Calkins

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THE PRESUPPOSITIONS OF A BEHAVIORIST PSYCHOLOGY

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1. The Natural-Science View of Mind

The popular idea of the soul is one which has been handed down to us from a time when it was believed that there were two different worlds. The one of these worlds was the world of external nature in which our bodies are born and grow and become old and die like the other animals and plants. This was called the physical or material world, the world of matter, and the body was regarded as a material object which had a beginning and an end. The soul, on the other hand, was conceived as belonging to the world of mind—the spiritual or mental or psychical world. And, in spite of the obvious facts of mental growth and decay, this spiritual part of our nature was supposed to be independent of death, indestructible and immortal. Some thought a new soul began with the beginning of each new body in the mother's womb, others that souls are eternal, appearing in different bodies at different times—reincarnations.

It is never wise to dogmatize about such matters, for the truth usually lies somewhere between the extremes of opinion. But there is one phase of the question we must not lose sight of: science is continually adding to the evidence which goes to show that there are no sharp separations in nature. Science encourages a belief in the unity of man with the rest of nature and the identification of all the forces of the universe in a single principle. Whether this principle shall be called

material or spiritual seems of less importance than the fact of the unity of all things to make a universe. Accordingly, the man of science insists, not only on the facts of the soul-life or mind, but on their interpretation in consonance with the rest of the laws of nature.

This leads him to demand evidence for any affirmation that is made with reference to the soul and to relegate to the realm of unproved hypotheses all views which have no basis except the desires and feelings of men. Such desires and feelings he admits are very real facts in the world, but so are illusions and superstitions and errors of all sorts. Just because he has faith in the ultimate triumph of truth the man of science demands that beliefs shall submit to the most rigorous tests. As a consequence, the idea of the soul is undergoing a profound transformation at the hands of the man who understands the meaning of scientific method.

The new idea is so different from the traditional one that to many it seems as if it were not soul at all. That science has thrown over a certain conception of the nature of the soul is certainly true, just as it has thrown over the belief that the earth is flat and that insanity is due to evil spirits. But that science has denied a place in the system of the universe to the facts represented by the word soul is not true. On the contrary there never has been so marked an advance in the field of mental science as during the past fifty years. The science of psychology has taken its place among the exact sciences.

The language in which the facts of mental life are described has undergone a profound change, so that those who have not been following the development of psychological investigation are apt to think there is no place left for the beliefs they hold. And perhaps there isn't if those beliefs are not grounded in experience. The man who believes that the earth is flat finds the modern sciences of physics and astronomy against him. The most the scientist can grant him is that the earth looks flat, and his belief is relegated to the realm of sense illusions. The scientist does not deny the appearance of flatness, but he commends the man who

holds this belief to that department of science in which such deceptive appearances are explained.

So in the case of the mental life or soul. If in the course of a more accurate observation of the facts of the soul life the duality of mind and body disappears as the flatness of the earth vanished before a deeper grasp of celestial mechanics, it must not be inferred on that account that the essential facts of the mental life have been denied. Indeed, the more psychological investigation discloses the nature of our psycho-physical life, the more marvelous such behavior becomes, until, in the minds of many students of these phenomena, it seems as if psychology had become the key to all the other sciences. Every fresh scientific inquiry feels this inter-dependence of its point of view with the whole system of organized knowledge.

In its first ambitious advances the young science was called by its critics the psychology without a soul. But this was a mistake. The word soul was no longer prominent in its nomenclature just as the word life was gradually dropped from the pages of biology, but the essence of the matter was there. The mental is now given a place in the system of things it never had before because it is being organized now for the first time into the very body of scientific method. It has become a principle informing all facts, whereas before it was conceived as a separate field sufficient unto itself.

Just as we have given up the primitive ghost or demon in the mountain and stream and tree, and as an explanation of insanity, just as we have given up the ghostlike essence called caloricity in the phenomena of heat, and as we are slowly sloughing off the idea of vital force in biology, so we must give up soul or mind or consciousness or ego or self, as distinct from a certain kind of behavior, in psychology. If it is irrational to postulate electricity in an electromotor or magnetism in a magnet except as an abbreviated description of the facts, it is likewise irrational to postulate a mind-force in a certain other kind of machine called an organism. We have achieved the impersonal point of view in the interpretation of stars and stones and trees and bacteria and frogs and guinea-pigs.

Our next step is to achieve it for the phenomena of human behavior. With this achievement will fall away the whole dialectic superstructure of epistemological dualities and entities which have lingered to confuse scientific method.

2. The Concept of Behavior

We humans are objects in space and time behaving in certain ways. In one aspect we are just *things* in a world of things, including stones and stars and atoms and electrons. We are different from other things: less like stones and apples than we are like whales and monkeys. We classify ourselves in the group called animals, mammals, primates, because we behave more like members of this group than any other.

For this reason we more narrowly define ourselves as *organisms*, and, among living things, as belonging to the group of animals rather than plants. We classify ourselves among organisms because organisms behave in a different way from mechanisms which we associate rather with the behavior of things. It may be, indeed, that an organism is only a more delicately constructed machine. In fact this is the assumption of exact science today. But there is such a marked difference of degree of adaptability between a living and a mechanical structure that we are justified, for practical purposes, at any rate, in describing the behavior of organisms as a distinct type.

But even this does not complete the account. We behave, not merely as things and as organisms, but as *persons*. Among living creatures there is a smaller or larger group that behaves in a characteristic way which leads us to speak of them as selves and, from certain points of view, we then ascribe to these creatures certain traits or capacities which we express by the terms mind, soul, spirit, consciousness, feeling, sensation, perception, memory, imagination, judgment, reason. These peculiarities of behavior may, indeed, be latent even in atoms and stones and stars. But, again, the difference of degree is so important—granting that it is only a difference of degree, which is the opinion of many men of

science—that we feel justified in describing the acts of persons as a distinct type of behavior.

Here, obviously, is a rich field of fact and a confusing wealth of theory. For we have set before us in this simple account some of the deepest problems of nature and of man. What is the true significance of these differences: between the unorganized and the organic, between the organic and the conscious? Do these types of behavior represent ultimate differences or simply different stages through which everything passes, sooner or later, in its evolution from the simple to the complex?

It is easy to slip into the fallacy of seeming to explain difficult points by accepting uncritically from the past hard and fast categories like mind and matter, subject and object, individual and society. Restricting our view to the natural history of behavior, and proceeding by the method of scientific observation and explanation, we are not justified in beginning our inquiry with any such fixed class-names. If these categories represent real differences this must be disclosed in the course of our study; we may not beg the question by assuming them at the start.

For the same reason we must be alert to the fallacy that lurks in the contrast of inner and outer, especially as contained in the concept of introspection which has been supposed to be the exclusive method of psychology. A natural history of behavior cannot recognize any special prerogative unless the facts clearly demand it. That mental behavior is of a distinct order, to be approached by a special method, is a point to be proved, not to be assumed.

And the same possibility of fallacy lies in the contrast between the individual and the social. Not that such a distinction has no meaning, but that the precise nature of the meaning remains to be made out; it comes properly at the conclusion of our inquiry rather than at its beginning.

For some time, in the field of scientific method, the need has been urged of a category common to the physiologist and the psychologist in terms of which the problems of bodily and mental function might be discussed without arousing

metaphysical prejudices. Such a category is action or behavior. The former is perhaps the more abstract concept, lending itself more readily to the discussion of the philosophical questions involved. The latter has the advantage of being a term of popular as well as scientific usage in describing the action of organisms. Books are appearing with the title or sub-title of Behavior, and under this term the authors are successful for the most part in discussing the activities of organisms without prejudging the question of the nature of the psychical and its relation to the physical. The behavior of microörganisms is being investigated in terms of action-systems in a way which does not preëmpt the field for either a mechanistic or a teleological interpretation of the facts.

It is too early in the history of the new point of view to predict the lines along which the two sciences will get together, but it is safe to say that there will have to be considerable revision of working concepts on the part of both the physical and the psychological branches of science. By this is meant that the recent insight into the energic nature of matter will in time inevitably affect the biologist's conception of the nature of what he calls an organism. Biology, in so far as it claims to be an exact science, regards the body as a complicated mechanism whose elements are to be understood in terms of the chemical and physical laws which hold for these elements outside the organism. So that if, for example, the study of the electrical properties of matter results in transforming our chemical and physical notions, and some form of an energic is substituted for the atomic theory, this dynamic view ultimately must reach into biology with transforming effect. In a similar way the conception of the nature of mind is undergoing reconstruction in psychological science, in part due to this same energistic theory which is transforming physical science.

The development of a congruous method for physiology and psychology will probably take place very slowly, however, because of the diverse historical conditions and techniques associated with the two sciences. Biology had its roots in the natural and positive sciences; psychology arose

as a branch of metaphysics and was long known as mental philosophy. But now that the basis has been laid for a scientific psychology, there is hope of its being possible for the psychologist and neurologist to get together in their work on the common problem. As has been intimated, this will involve a revision of psychological conceptions on many fundamental points. That this is already taking place is evident from recent tendencies in psychological thought. Consciousness is coming to be stated more and more in terms of action, in terms of the motor aspect of the organic circuit instead of in terms of the sensory aspect which was the tendency with the older writers. Great emphasis is now being placed on the motor character of attention, on the dynamogenic nature of ideas, on ideomotor impulses, the tactile-kinæsthetic imagery as the carrier of meaning, on emotions as the vestiges of motor attitudes, on the growth of voluntary movement, and on the reconstructive function of thought. It is coming to be recognized that the distinctions between reflex and instinctive activities are not fundamental, and that the sharp line drawn between instinct and intelligence will fade away before a more adequate analysis. Perception is defined as an attitude toward the object perceived, a reverberation within the sensorium, not merely of the present stimulation, but of stimulations in the past preserved by the chemical retentiveness of the tissues. As Alfred H. Lloyd has said, "Nothing is so much needed at the present time as the adjustment of the science of abstract thought to the science of organic action, and every little hint as to how this adjustment can be brought about cannot but be at least a little help. The evolution of consciousness must be almost meaningless until the simplest case of accommodation as seen by the biologist is identified with the most perfect case of abstract thought that the logician knows." [1]

3. The Concept of Introspection

One of the leading problems which has agitated psychology on the side of method has been the question of how it obtains

[1] Psychol. Rev., 1896, 3, 426.

its data. Springing, as it did, chiefly from philosophy, the new science naturally carried over into its procedure certain assumptions which upon subsequent examination have proved more than doubtful. One of these, as we have seen, is the assumption of the existence of two orders of reality, the physical and the psychical. This, under analysis, has been refined to a mere concomitance of phenomena or, indeed, in the view of an increasing number of writers, has completely evaporated as a valid distinction. As long as some form of psychophysical parallelism prevailed, whether a true parallelism of different orders of existence or a mere parallelism of manifestation, it was inevitable that psychological method should be affected thereby. This influence has shown itself most in what is known as the doctrine of introspection.

This doctrine, until quite recently, has been held to be the distinctive characteristic of the science. Psychology has been supposed to derive its subject-matter from a different source and by a different method from the other sciences. The data of all other sciences are discovered by direct inspection of facts in the objective world of space and time, *i. e.*, by *extro*-spection. Psychology, on the other hand, was the one science which was supposed to find its facts by *intro*spection, *i. e.*, by an inside view of this second order or aspect of reality called the psychical or mental.

If, however, the grounds for postulating the existence of more than one order of reality are questionable, it follows that the presumption is against this uniqueness in the nature of the data and methods of psychological science. If we stick closely to the concrete facts of the universe as we find it outside and inside our organisms, it appears that all we are entitled to say is that while certain facts are equally ascertainable by all observers, certain other facts, namely those within the organism of each individual, are more easily ascertainable by the individual concerned than by outside observers. But this ease of access, because of nearness to the facts, is not a sufficient ground for characterizing such observation as a unique method or the data thus secured as differing in kind from other data secured by the objective method.

What we observe in so-called introspection is usually but the inner bodily beginnings, hidden from outer view, of the same behavior which in its outer overt manifestations is described by external observation. This obviously is merely a difference of accessibility of the facts, not a difference in their nature. A nascent response is just as truly an objective datum as the completed performance.

All data of science are data of individual inspection in a sense. Every scientific inquiry begins in some individual's experience and proceeds by an examination which is sustained by the interest and purposes of that individual inquirer. Not even in the most rigidly experimental method is it possible absolutely to exclude or control the personal equation. The individual's outlook upon the universe inevitably colors the descriptions he gives and the conclusions he draws. The greater part of scientific technique is just for the purpose of checking up results in terms of such factors, and we speak of the results of an inquiry as scientific just to the degree that we succeed in reducing this personal element to a negligible quantity.

We may go even further and admit that all the information I derive by what is called external observation—for example, by the use of the distance-receptors, the eye, the ear, and the nose—must be translated into terms of that more immediate information that I derive from my own inner bodily processes—derived, in this case, through end-organs in muscles and viscera and semicircular canals. But this means merely that the somatic organs of discrimination are genetically older than the cephalic distance-receptors, and therefore are the primary carriers of the meaning of the situation, not that the facts ascertained in the latter case belong to a different category. It is the same sort of situation as when, with an imperfect knowledge of a foreign language, I find that I must first translate the unfamiliar symbols into terms of the mother-tongue.

The root of the matter is here. It see that you behave in a certain way that resembles the way I behave when I am in pain. In the same way I infer that you have many other

experiences. With somewhat more difficulty, if I were blind, I might make the same inference from the tone of your voice; or if I had the nose of the dog I might make the inference by the sense of smell. If my only knowledge of your behavior were through the senses of touch and movement, I could know your behavior only as I, so to speak, participated in it—acted with your acts and sympathetically pulsated with your attitudes. Helen Keller secures an approximation to this through the refinement of her tactile and kinesthetic senses in the use of the manual alphabet, placing her fingers on the speaker's lips, passing them over a surface to explore its meaning, etc. Ultimately I could know your behavior only as I entered into the experience you are having, only as my nervous system were commissured into yours. If for seeing could be substituted touching and particularly hefting, pressing and other kinesthetic experiences, the distinction of inner and outer implied in the so-called introspective method would not exist. In other words, if the intro*spect*ive were reinterpreted in terms of an intro*tact*ive method, vision in terms of touch and movement, the supposed difference between the two kinds of data would disappear.

That this is true is shown by the fact that we do not as readily ascribe consciousness to the ant or to the oyster or to the tree as we do to the dog or the horse or the man. We cannot participate in the experiences of these creatures in terms of mandibles and tentacles and waving leaves and swelling root-hairs as we can in the case of creatures with behaving organs more like our own. The problem vanishes to the degree that we put ourselves into the attitudes common to these lower forms and seek to describe and explain behavior in terms of contacts and movements. If we could quiver with the jelly-fish intimately enough really to know its behavior, we should simply be describing our common experience, as we already do in our objective sciences of the phenomena of the external world. There would be no problem of introspection and inference to other consciousness, no problem of inner and outer, no problem of mind and matter, no problem of unshareable individual consciousness on the one hand and shareable social experience on the other.

It is possible, of course, to have a science of individually observed facts only on the assumption that they are typical phenomena, *i. e.*, phenomena similar to what other similarly situated individuals observe. Science is the statement of observed uniformity (universals) among particulars. But there is no problem here which is not encountered everywhere in science: it is just as true that no two blades of grass or grains of sand are precisely alike as it is that no two psychological facts are alike. The data derived from the individual's observation require to be checked by the data derived from observation by other individuals here just as they do elsewhere. The scientific standpoint is always the standpoint of the observer, the third person's point of view. The first person's point of view is as important and valuable as one pleases; it may be largely determinative of behavior: but it is not science. Inside facts must first be made outside before they acquire the status of scientific data. This is commonly expressed by saying that the standpoint of science is objective, experimental, and mathematically exact. No merely particular, private, individual fact has any scientific value save as a point of departure for research.

4. Current Conceptions of the Mental

Various attempts have been made in recent years to formulate a definition of mind from the natural-science point of view.

The one which has aroused the most discussion is known as the Relational Theory of Consciousness, first formulated by Woodbridge. According to this view consciousness (and by implication mind, which in this discussion is not distinguished from consciousness) is not a substance, nor an attribute of a substance, but a relation. "Consciousness is the result of the interaction between the organism and its surroundings. . . ." In the sense organs and central coördinating mechanism we find the two characters of consciousness—its diversity and its unity. "An organism so situated that it should be in differentiated interaction with the specific differences in the world about it, but which should, none the

less, react in a unified and coördinated manner no matter how it might be stimulated, might well be defined as a conscious organism. Its consciousness would be a relational system integrating and unifying its differentiated interaction with its surroundings. Furthermore, its consciousness would naturally be marked by many of the characteristics usually attributed to consciousness. It would, for instance, be what we call individual and personal, and, being unified, it would present features often ascribed to a self or mind." [1]

Similarly Montague says: "If consciousness is at length to submit to the same scientific treatment that we accord to other phenomena, *i. e.*, if it is to be equated to a mode of relation between things—it must inevitably be regarded as secondary to those things." Consciousness is a 'mode of connection between the organism and its environment.' [1] Consciousness is 'a relation existing in a material nature along with other relations of space and time.' [2]

Singer says that the mind or soul must go into the same methodological oblivion that has already received the entities once supposed to exist under the names of caloricity and vital force. "Life is no longer a thing to be inferred from behavior; it is behavior. . . . Disembodied life has been placed among the myths." [3] "Consciousness is not something inferred from behavior; it is behavior. Or, more accurately, our belief in consciousness is an expectation of probable behavior based on an observation of actual behavior, a belief to be confirmed or refuted by more observation, as any other belief in a fact is to be tried out." [4] "I don't know what life means nor what consciousness means," but "neither stands for an eject forever veiled and hidden in the land beyond experience. . . . I regard my own mind as behavior quite as frankly as I take my fellow's mind to be nothing else." [5]

[1] *J. of Phil., Psychol., &c.*, 6, 449–450.
[1] *Ibid.*, 2, 313–314.
[2] *Ibid.*, 4, p. 377.
[3] *Ibid.*, 8, p. 186.
[4] *Ibid.*, p. 183.
[5] *Ibid.*, p. 184.

According to Hollingworth "all sciences are studies of behavior." Psychology is the 'science of behavior of statistically variable experience.'[1] "The physical world, with which the physical sciences deal, and for which are developed the various shorthand symbols by which we designate the objects as independent of experience, consists of those experiences which are statistically common. The independence of these objects, their stubbornness, their resistance, their objectivity and naturalness, these all are not unique characteristics which suffice to split experience in two, they are merely various and interesting ways of stating the same statistical fact. The dependence, the subjectivity, the personal character of other experiences, the so-called mental order, are merely literary terms which express their statistical limitations and their consequent vagueness and complex conditions of appearance."[2]

Bode (in 'Creative Intelligence,' by Dewey and Others, in the chapter entitled 'Consciousness and Psychology,' pp. 244–255) says that consciousness is 'a name for the control of conduct by future results or consequences.' It is 'just a future adaptation that has been set to work so as to bring about its own realization.' "Conscious behavior, as contrasted with the mechanical character of the reflexes, is essentially experimental. . . . All experience is a kind of intelligence, a control of present behavior with reference to future adjustment. To be in experience at all is to have the future operate in the present. . . . It is this relationship of present response to the response of the next moment that constitutes the distinctive trait of conscious behavior. The relatively unorganized responses of the present moment, in becoming reflected in the experienced object, reveal their outcome or meaning before they become overt, and thus provide the conditions of intelligent action. In other words, future consequences become transformed into a stimulus for further behavior. . . . If it be granted that consciousness is just a name for behavior that is guided by the results of acts

[1] *Ibid.*, 13, p. 187.
[2] *Ibid.*, p. 186.

not yet performed but reflected beforehand in the objects of experience, it follows that this behavior is the peculiar subject-matter of psychology. . . . A quality, such as 'sharp' or 'hot,' is not mental or constituted by consciousness, but the function of the quality in giving direction to behavior through certain changes which it undergoes *is* consciousness. . . . Psychology, therefore, is properly a study of the conditions which determine the change or development of stimuli; more specifically, it is a study of the conditions which govern such processes as those by which problems are solved, lessons are memorized, habits and attitudes are built up, and decisions are reached."

Sellars says: "Consciousness is a variant and seems to have meaning only in an organism's stress relations to its environment." [1]

Perry says: "Mind *is* behavior, or conduct, *together with* the objects which these employ and isolate. . . . A mind embraces certain objects, or parts of the environment, with which it deals in its own behalf." [2]

Dewey says the mental is the fragmentary character or aspect of the real. "Any given set of facts of which there is an idea is not yet fully real in itself. . . ." That is, the mental, consciousness, sensations, ideas, are reality demanding completion or, at least, transformation into something else. "It demands precisely its own further requalification." [3]

"Where, and in so far as, there are unquestioned objects, there is no 'consciousness.' There are just things in their factual relations. When there is uncertainty, there are dubious suspected objects; things hinted at, guessed at. Such objects have a distinct status, and it is the part of good sense to give them, as occupying that status, a distinct caption. 'Consciousness' is a term often used for this purpose." [4] The only reason we come to regard ideas as private and personal is that in most biological adjustments these dubious suspected aspects are more apt to fall within

[1] *Ibid.*, 5, p. 238.
[2] *Ibid.*, 6, p. 175.
[3] *Ibid.*, 4, p. 257.
[4] *Ibid.*, 5, p. 375.

the organism itself or those which do so fall are emphasized since they serve not only to present the problematic character of the situation but supply also the instruments for its reconstruction. These partial aspects, in the absence of the ability to serve as factual realities themselves, come, as symbols, to stand for other realities; that is, they become ideal realities, ideas. "So treated they are tentative, dubious but experimental, anticipations of an object." These 'intra-organic events' thus are 'inchoate future cosmic objects in process of empirical construction.'[1] "Ideas, sensations, mental states, are, in their cognitive significance, media of so adjusting things to one another, that they *become* representative of one another. When this is accomplished, they drop out; and things are present to the agent in the most naïvely realistic fashion."[2]

5. The Distinguishing Marks of the Mental

If we proceed with mind, then, as we do with other phenomena—that is, in the spirit of natural science—we can only ask what particular characters behavior presents when it is mental, which distinguish it from vital or mechanical or chemical or electrical or other behavior. Proceeding in this way, the following factors appear as distinguishing marks of the mental: (1) The capacity of an organism to use one part of its experience to control another, *i. e.*, behavior is mental in its aspect as inducing further behavior; (2) especially the capacity to control experience by the use of symbols, particularly words, *e. g.*, in human behavior language is the chief instrument in the induction of further behavior; (3) the social-individual amplification of experience: social interchange, when progressive, is typically the induction of new social values through the medium of individual variations and experimentations.

1. 'Learning by experience' (using the word experience in its primary meaning as having to do with behavior which is experimental in its nature) presupposes, within a given total of

[1] *Ibid.*, p. 381.

[2] *Ibid.*, 2, p. 325.

activities, that one action-system conditions another in such a way that the first is a factor in bringing about a change in the second. That behavior may not manifest itself in such a way without invoking subjectivity in a metaphysical sense it is for those who deny it to prove. Certainly the mechanism of summation of stimuli and inhibition of response is objective enough in the scientific sense, not to require any special defense against metaphysical prejudices.

There do exist relatively unstable as well as stable vital equilibria within organic complexes and these exhibit, under certain conditions, the phenomena of selective attention in the definition of the stimulus and selective inhibition in the definition of the response. In so far, therefore, as a total overt act is mediated by an incipient act which ordinarily is but a part-process within such a total act, we have, in simple terms, phenomena of induction of response sufficiently distinctive to deserve mention under a separate rubric—the mental.

The very sentence I am here writing presents an illustration of the principle. Each phrase and word is indicative of the meaning which lies beyond and at the same time within itself. The full import of the first word is not found until the last is reached. Nor can the last be understood apart from the first. Within the whole represented by such a bit of behavior there is a vicariation of part-processes absent from what we call mechanical or non-mental operation. And if we knew more about the behavior of horses and fishes and birds and paramœcia, doubtless we should see the beginnings of the operation of this principle there. Jennings seems to find evidence of it in his researches on the microscopic forms. The problem of psychogenesis is the experimental isolation of these part-processes.

An organism in a context in which it is not adapted is in a state of disturbed vital equilibrium. Its receptors and effectors together with its coördinating adjustors are thrown into a state of excitement which, from the point of view of the subsequent readaptation, is called experimental. They demand, as it were, a certain sort of environment to function

in, which as a fact is not present, or is present only as a possibility in the nascent capacities of these organs to modify conditions. In so far as these capacities through this sensorimotor mechanism, as an upshot of this experimental activity, do begin to reorganize, not immediately the external environment, but the 'set' of the organism toward a putative environment, we speak of the activity as purposive. This likewise is what we mean by its being mental.

Purposive or mental behavior, therefore, is behavior which has engendered or is in process of engendering such a 'set' or pattern of habit-systems *now* that given results will flow from it *then*. I purpose (pro + pono) when I place before me such conditions as will determine later conditions, in other words, when I control my subsequent act of attention by my present placing, articulating, fixing of the relevant motor mechanism.

I, for some reason, image a drowning man and find myself wrung by an involuntary spasm of strangulation, from which, however, I free myself as I realize that this is but an imaginary, not a real, situation. In other words, I momentarily become the victim of some predetermined 'set' in my nervous system which, in a suitable setting, would lead to adaptive movements, but which in the absence of such a setting falls to pieces in the arrested stage of merely (or chiefly) implicit behavior.

A ball comes flying through the air in my direction and I involuntarily prepare to catch it. Here likewise there is a predetermined 'set' of the organism, nerves and muscles, in this case carried out to completion in an adaptive movement. Both are purposive and mental, both are of the nature of incipient performance, predetermining stages in a progressive process of adaptation, in the one case carried to a certain completion within the field of tensional stress represented by the interplay of receptors and effectors, in the other case arrested and shunted off in an earlier stage vaguely alluded to usually as central processes.

2. The phenomenon of the mental appears unmistakably wherever a creature, by the device of language-symbols, refers

to itself or employs itself, that is its own present or past experience, as a factor in some new behavior equation. As long as behavior is relatively adequate it remains on the plane of the physical or at most the vital. But when a given confrontation of stimuli presents the character of a crisis, and in this emergency the existing behavior equipment is insufficient to meet the situation, it figures as a factor only in a wider behavior complex which is emerging.

We should expect to find what we in fact do find, that the mental is a more or less faithful mirror-picture of the material world—an imperfect picture, faint and indistinct in spots, distorted and exaggerated in others, as would be the case where the symbol is imperfectly performing its function of substitution for the symbolized. The very language in which the mental is primitively described shows at once the initial unity and the induced duality of this process: this lingers in the concepts of mana, manitou, and participative consciousness now coming to light in anthropology.

The creature that first found the word 'I'—a symbol to represent itself—must surely have paused in astonishment before this duplication of itself. What wonder if it demonized its own half-conscious creation and built up a second world of its fancy in which symbolically at least it might live the life of freedom and accomplishment it found so difficult to achieve in this. If the epiphenomenon of consciousness originated in some primal grunt of satisfaction, as thinking is supposed to have originated in subvocal talking, then the whole problem of a separate realm of mind falls away, and our investigation narrows down to the specific characters we shall agree to ascribe to behavior when it exhibits a certain degree and kind of complexity.

3. That consciousness is essentially social in its nature has been emphasized in recent years on all sides. The interlocutory origins of thought are as evident as the sympathetic origins of emotion. And that the perfecting of language as an instrument of vicarious experience has played the chief rôle in the building up of the superior mentality of man no one doubts. It is perhaps not quite so clear, and therefore

deserving of accentuation, that the presence of mentality anywhere is based on the same principle, whatever the peculiar form of the symbols. A tropism rendered plastic by unwonted attrition in adjustment, a delayed reflex, a thwarted instinct—any reorganization of behavior that throws one act into the indicative or demonstrative relation to another act—a gesture, a grunt, an animal cry, a facial expression, protruding claws or bristling mane, as truly as the articulate word, is the material of which mentality is made. And these substitutional modes doubtless functioned as between units in a social matrix before they became convolved in the consciousness of the individual.

Mind thus is not something superadded to behavior; it *is* behavior of a certain sort. It is behavior in which certain objects which serve as excitants are undergoing experimental reconstruction into stimuli adequate to the incipient response. It is behavior in which certain attitudes are undergoing reorganization into adequate attentional discrimination of the response. What in psychology has been known as sensations and ideas are but the sharpening of the stimulus and response in terms of the incipient reorganization of behavior set up within the total circuit by some shock of relative disadaptation. The mental, with its retinue of ramifications into consciousness and attention, sensation and image, affective and cognitive modes, is but a name for behavior of that sort which demands something else than itself for its own completion. This new other-referring and other-demanding quality in an act, this indicative or demonstrative, this symbolizing, vicariating, inducing, representative character is the distinctive mark of the mental.

Summarizing, we may say that mentality or mind is a name for the fact of the control of the environment in the interest of the organism through the interaction of inherited capacities and acquired abilities. Mind is not an organ or function or faculty over and above the mechanism of behavior; much less is it an entity of a distinct order or a parallel aspect: it is a relation. Like gravitation or evolution, it is merely a generalization from certain facts, the state-

ment of a type of relationship. The word mind, like the word habit or the word instinct, stands for an observed uniformity of events; it is a class-name for an assemblage of particular facts of adaptation and adjustment in behavior. Mind is the name for a fact of control-in-crisis which takes place under certain conditions in the life-process. Psychology is the effort to describe and explain such phenomena. We may provisionally define psychology, accordingly, as the science of the behavior of organisms in so far as they exhibit mentality. Behavior—not all behavior, but behavior in so far as it presents the character of the mental as distinguished from the chemical, the physical, the vital. Mentality—behavior in its aspect as inducing fresh forms of itself.

THE SIGNIFICANCE OF BEAUTY[1]

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Human beauty is something which is perennially celebrated in poetry, in song, in romance, and in the petrified conception of the sculptor, but less frequently considered in the cold analysis of science. We are usually content to leave the topic to the artist and the lover, as one of the interesting and thrilling, but non-essential, matters of life. I wish to suggest a different conception of beauty: a conception of beauty as something which, whatever its importance for the individual, is for the race and for civilization of such profound importance that no other fundamental consideration of human welfare and progress can be divorced from it. I shall not touch upon the theme with the golden fingers of the artist, but with the unemotional digits of the psychologist. To some, without doubt, this procedure will seem as sacrilegious as the piercing of the anatomist's knife into the dead human form; but where the welfare and progress of humanity are at stake, even these brutal methods must be employed.

Beauty is a term of variable meaning; in fact there is a group of terms—handsome, pretty, attractive, charming, etc.—whose exact relationship is often discussed, and never settled. The way in which I use the term will not be acceptable to many persons, but one may reformulate my conclusions in his own way, using whatever terms he chooses, and the validity of the conclusions will not thereby be affected. I think it will be agreed, when I am through, that I have been discussing something rather definite under the name of beauty, and I hope further, that it will be conceded that, after all, what I have been discussing is that which in

[1] An address delivered before the joint session of the Southern Society for Philosophy and Psychology and the Association of Physical Directors of Women's Colleges and the faculty and students of Randolph Macon College, Lynchburg, April 13, 1917.

the common, and therefore vital, usage is actually designated by the term.

The familiar proverb tells us that "beauty is only skin deep," which nicely exemplifies the mendacity of proverbs; ugliness, it is true, is often skin deep, but beauty, never. Beauty, as I hope to be able to show, is something which depends upon the whole organism.

The conditions of beauty are in part negative, in part positive. That is to say, there are certain conditions which a person must satisfy in order to be classed as beautiful, yet which do not in themselves contribute to beauty; other conditions, such that their fulfillment constitutes beauty, or at least constitutes a certain element in the total beauty. Among the negative conditions are, for example, the lack of deformity. A hunchbacked woman or a baldheaded man is debarred by the deformity mentioned from being classed as beautiful, but the fact of having a straight back or of having hair on the head is not necessarily in itself a positive element of beauty. The negative condition is one which may be fulfilled, and yet the individual not be beautiful and not even have the corresponding detail of beauty. The positive conditions, on the other hand, are those which taken together in their fulfillment cause the person to be classed as beautiful. Some of these details may be present, and yet on account of other negative or positive factors, the total may not constitute beauty. Nevertheless we say that, in these details at least, a person does possess beauty.

This distinction between positive and negative elements, I am well aware, is not fundamental; it is at best a distinction of degree and convenience. But it is a convenience, for purposes of presentation at least, and we may make use of it while noting the fact that too great dependence upon it is fallacious. I shall consider first, therefore, the general negative conditions in order to clear the way for a treatment of the more detailed conditions, which, although involving both positive and negative elements, are better treated from the positive rather than the negative point of view.

I shall consider herein, primarily, only visible details.

Qualities of voice, peculiarities of odor, tactual details, and so on, I shall notice only in so far as they are directly associated with visual characters. This is in accordance with the usual practice which makes beauty essentially a visible phenomenon and only secondarily a phenomenon which appeals to other senses.

General Negative Characters

1. Signs of race. There are certain negative details of stature, feature, color and movement and habits which are important because they indicate in the first instance a race or species of the human family against which, for reasons which may be instinctive or due to education, there is a prejudice. Facial proportions, for instance, which in themselves have no value, may yet indicate or suggest a branch of the human family against whom we entertain a certain bias. If we despise the Irish, an Irish caste of countenance cannot be beautiful to us. If we have an antipathy to the German or Russian or the French people, the type of face which suggests these people, even though there is no indication of actual blood of the race, is a factor making against beauty. The commonest instance of this sort of negative condition is found in the negroid characters. Here, where the suggestion or indication is of an inferior race, the negative condition is especially important.

2. Signs of disease, deformity or weakness. Any indication, not merely of physical weakness, but even in some instances of mental or moral weakness or disease is of decided negative effect. One who looks like an imbecile or like a criminal is never beautiful; one who seems to have, or suggests, a deadly disease, is to that extent lacking in beauty. To a certain degree, these mental and moral standards are relative to the grade of the observer. A weak-minded person has not the objection to the weak-minded person of his own grade that the more normal person has, but I suspect that the person of low mental grade has a certain preference for the normal person. As regards disease and deformity, there is no question. A hunchbacked or baldheaded man regards his characteristic as a decided bar to beauty.

3. Significant deviation from the average is a negative characteristic, even if the deviation cannot be classed as a "deformity." Dwarfs and giants, exceedingly thin and unusually broad individuals; those whose legs are too long for their bodies, or *vice versa;* those whose ears are misplaced, or whose hair is of an unearthly shade, are ruled out by their oddity, regardless of what these peculiarities signify. They may be good, clever, or admirable, but never beautiful.

These details are in part relative. Among certain African tribes, whose men are uniformly over seven feet tall, and as thin as a rail, a normal Anglo-Saxon is probably not beautiful. Among other African tribes, and certain islanders of the Pacific, a woman is not considered beautiful unless she reaches a degree and a distribution of fatness which makes her either repulsive or comical to European eyes. This relativity is, however, only superficial. The type which is highest in value tends to approximate the European type, wherever the European type becomes known. All dark races prefer white skin, and it is a general rule that the female of the inferior race prefers the male of the superior race to the male of her own race, no matter how striking the difference. That the inferior male considers the superior female more beautiful than the female of his own race is indicated everywhere, and clearly demonstrated among the Turks.

Deviation from the common type, then, is a drawback only when it is not a deviation towards the acknowledged superior type of another race. The conservative dislike for the unusual in general is tempered by approval when the unusual is clearly a mark of racial superiority. This will find its ready explanation when we consider the positive side of beauty.

4. Misplaced sex characters. A specific form of the abnormal, but one which is important enough to justify separation from the foregoing class, is the possession by individuals of one sex of characteristics properly belonging to the other. This is an invariable negative qualification in the eyes of healthy observers. The effeminate man and the masculine woman can be beautiful only to the moral pervert. The

importance of this indication is very great, as we shall see later, and however little it may mean consciously to a given individual, the habit of reacting against it has been strongly developed in the human race.

Detailed Characters of Beauty

So much, in brief, for the general negative characters of beauty. We come now to more detailed characters, which have on the whole a positive value, although some of them have negative aspects as well.

1. *Stature.* From the point of view of the female, the male must be large, although not a giant, since, as we have seen, too great a deviation from the usual is a negative condition. I have at various times overheard women, who were discussing the relative handsomeness of two or several men, settle the point by such an observation as '*A* is fully an inch taller than *B*.' By carefully put questions I have succeeded in eliciting a considerable amount of information on this point without revealing the actual purpose of the interrogation. For example, if I inquire of a woman concerning the handsomeness of a man who has a general combination of desirable and undesirable characteristics, but who is a trifle below medium height, I very frequently obtain, in her first statement, a criticism of his stature, followed by a consideration of his other attributes; indicating that in her estimation size is of paramount importance. The determining factor is not, of course, mere height, but height combined with lateral development not deviating markedly from the average proportion. The tall man of bean-pole build is not considered attractive. Yet, a positive element of height can outweigh a considerable element of disproportion, and a taller man, whose proportions are in themselves worse than those of a shorter man, is usually considered the handsomer.

This preference for stature undoubtedly harks back to more primitive times, when it was above all important that man should be a fighter and hunter, in order to secure food for his wife and children, and protect them against wild beasts and against the designs of other males. Especially

was this important during the periods when the woman was pregnant, or nursing a child. It is highly probable that in ancient times the negative rule against abnormal size did not apply, since every increase in physical power, even if carried to the extreme of gigantic development, was a distinct advantage.

It is sometimes alleged that the woman's preference is not for the *large* man in an absolute sense, but for the man *larger* than herself; either because of a natural wish for a husband to whom she is inferior; to whom she can give a tribute of worship and deference; or else, that it has developed through the necessity of the greater strength on the part of the man in order that he might capture the woman, and carry her away from her parental habitat, to his own dwelling. Both of these suggestions are highly unplausible. Marriage by capture, although a good hypothesis for popular writers, probably never was at any time an institution of any more importance or actuality than it is at the present day. Psychologically, the theory is based on the assumption that woman is naturally opposed to the marital relation, which assumption is a merry jest, to say the least. Historically, there is no evidence for the theory of capture except as a limited and temporary phenomenon. As for the supposition of an unexplained instinct to prefer a dominant partner, I see no support for it, except in so far as the practical consideration I have advanced may itself lead to this preference as a secondary manifestation. It is true that there are women today who openly state that the mates they want are those who can completely dominate them; and that such potential masters are the only men who interest them. These cases (a number have been directly reported to me) are not all to be explained on the same basis, although the primary factor in every case is the admiration for the strong man. In some cases, the preference is distinctly a pathological development; in others, it is pretended by the woman as an explanation for the fact that men are not interested in her. In many cases, however, the preference is the expression of an arbitrary standard which is manifested usually in less egotistical ways.

Where a scale of values is accepted, there is commonly a more or less explicit adoption of a minimal acceptable value; the stronger man is the more desirable; a man who measures up to a certain minimum will be acceptable. In most cases, the minimal standard adopted is the father, a brother, or some other impressive individual in real life or in fiction. In the case of a strongly egotistical woman, who sets a high value on her own potentialities, the standard is herself; the man less forceful than herself is below the minimum.

In this, I seem to be confusing physical strength with various sorts of power; perhaps I am; but, as I am trying to point out, the basis of power is muscular, and admiration for physical prowess still retains a primacy when it is a matter of the fundamental attraction of the woman to the man; and all I am trying to establish at this point is that there is no primary desire of the woman for a man who is able to dominate her physically. On the contrary, the woman would prefer, if other considerations did not prevent, the mate whom she can control physically and in every other way, for the *instinct to dominate* is inherent in every normal human being.

Under present conditions, the preference of the large woman is accentuated, and that of the small woman reduced, by social factors, especially the fear of ridicule. The weakness of the small man is made conspicuous by the contrast with a giant wife; compared, on the other hand, with a diminutive wife, his inefficiency is less emphasized.

From the point of view of the male, the question of stature is less simple. There seems to be no general preference for small women or for large women; but a truly relative preference for smaller women. Of course, I am well aware that there is a wide range of individual preferences, not all of which are explicable from available data; but I am speaking of generalities, which are certainly discoverable, in spite of individual differences. This general relative preference in the matter of stature is complicated by the curious double preference of the male, which is so strikingly demonstrated by theatrical studies, and to which I shall make brief reference later.

The primitive reason which leads woman to prefer a large man has no correspondence in the necessities of the male. The male has not the need for protection at certain periods which the woman has. While the addition of a husky female to the savage's fighting force would seem to be a prime advantage, the advantage is largely lost because at the precise times when the aggressive resources of the family are most fully needed, the woman is not in condition to exert her strength, without serious injury to herself. The physical strength of the woman is not to be counted on, and hence the stronger woman is not a greater asset to the family, and hence no more desirable.

It is true, there have been and still are, races in which the physical strength of the women has been counted on, especially for agricultural duties (*e. g.*, the American Indians); and among them, possibly (I am not certain on this point), stature has been a mark of beauty. But where female strength is counted on, it is necessarily utilized at times when grave damage is done to the woman, and those races which have counted on it have gone down. The races which have early developed *chivalry*, as we may well designate the protective attitude, are the races which have developed civilization, and which must continue to dominate the world unless civilization is to be abandoned, and the human race plunged downward into bestial degeneracy.

Stature, therefore, except in so far as it may be involved indirectly in some of the factors which I shall yet consider, is not and cannot be a mark of female beauty in a civilized race. On the other hand, by this very fact, the preference for a partner whom he can dominate is allowed full sway in the male. The woman would have the same preference, as I pointed out a moment ago, were it not checked by other factors.

I may digress for a moment, to remind you that in a family one person must control. This is not a theory, but an empirical fact against which argument is futile. Economic conditions which are as yet but dreamed of, especially those conditions which result from the greater and greater

use of machinery, may in future change this; but it was the law of the primitive family, and even yet we have not reached a stage of civilization in which a joint legislative authority is possible. In the past it has been the male who has controlled, but that may be changed in the future. It is true that Bachofen and others have tried to establish the doctrine of the matriarchiate (the rule of women) as the primitive family system, but the confusion on which this theory was based has been readily exposed. Never in the history of the globe did woman have the political and social power she holds today, and suffrage cannot increase it.

2. *Bodily proportions.* In modern civilization there has grown up an immodesty which was lacking in more ancient cultures. We are ashamed of our bodies. Whether the practice of concealing the body is the cause of our uncleanness of mind, or whether our obscenity is rather the cause of the concealment, is a debated question. Whatever may be my general estimate of the Japanese, I cannot but admire their wonderful cleanness of mind, which makes for them clothing a detail which has no bearing on modesty.

Among the Greeks, who, as you know, were in many respects more pure-minded than we are, bodily conformation was an important detail in beauty. And, in fact, it is today amongst us, both in a shame-faced way in daily life, and more creditably when we throw off our prudishness in the presence of plastic and pictorial art, and in the theater. We are skirting here a vital and pressing problem of the present moment, on which I should like to take the time to make you face some problems we all tend to ignore, but I must not digress further.

Our standards of bodily development are still, in the main, Greek. There are certain proportions which are judged both by the artist and the layman to be the ideal of beauty. In this we are of course swayed largely by the limitations of our education, which on these matters is artificial; probably there would be a greater difference in racial ideals, if conditions were more natural.

The simplest explanation for the accepted ideal of form

would be that it is the average form of the healthy individual. This explanation, I think, is not supportable. Among the Greeks and Romans, for example, the ideal ankle, for a woman at least, was a *small* ankle, not a medium-sized one. Among us, a small foot has been desirable; so much so that women have been compelled to wear shoes which, by raising the heel several inches, make the shoe about two thirds the real length of the foot and so make the foot seem shorter, or at least it did until the recent raising of the skirt has brought the artifice out where it cannot be overlooked. One of the most important and desirable effects of the permanent adoption of sensible clothing by women will be the allowing of the foot to retain its natural form. Of body-form, which is by rights the fundamental consideration in beauty, I shall say nothing further, because our standards are so obscure. The subject is in need of thorough investigation by the methods of comparative anatomy, and above all, of social psychology.

3. *The features.* Whatever the cause of our concealment of the body, it has led to an emphasis on the anatomical details of the face which could not be found in more primitive times. Leaving out of consideration the general shape of the face and head, which are probably important mainly as racial signs, we may consider briefly the chin, the nose, the eyes and the ears.

That there is a preference on the part of both sexes, and in the consideration of both sexes, for a well-developed chin, is a matter of common knowledge. The reason for this preference is less evident, and in fact I can here indicate only a strong probability. Racial factors are involved, of course, but there seems to be a more general foundation which is vaguely involved in the commonplace statement, that the possession of a chin is one of the conspicuous points which differentiate man from the beasts. This is obviously true; the vital question is, what are the direct consequences of this structural peculiarity? This question can be answered by reference to comparative anatomy and to the psychology of the thought processes. The projecting chin gives room in

the mouth cavity for the human tongue, which is strikingly different from the brute tongue. The tongue of the lower animal is a long thin strip of muscle; the tongue of homo sapiens is a thick muscular mass. A somewhat exaggerated comparison is to a leather strap, in one case, and a frog seated in the mouth in the other case. We have now advanced the question one step farther, to ask what may be the advantage, if any, in the form of the human tongue.

The animal tongue is certainly just as well adapted to the purposes of obtaining and preparing food, as the human. In some cases, it is even more efficient. But the human tongue is an important instrument in the production of the most human of all attributes, *language*. Language is not merely the means of communicating thought; it is, as philologists have long known, and as psychologists have been forced somewhat unwillingly to admit, the principal means of thinking. While it is possible to think without language, languageless thought is primitive and inefficient in the complex conditions of civilization, and it is by no means an exaggeration to say that the development of language is a large part of the development of thought.

Of course, it is not to be said that in any specific case a large tongue is an index of efficient thinking, or that a relatively smaller chin indicates necessarily a relatively smaller tongue, or that the converse of either of these propositions is true. But on the whole, the development of the chin is concomitant with the development of thought, and hence, in races or large groups, an index of mental development. It is worthy of note here, that the marks of beauty will be found throughout to be these *generalized characters*, which in specific cases may not be associated with the fundamental factors which have made them important.

The nose and the mouth are beauty-characters which are probably more exclusively racial in their significance than the chin. The broad flat nose and the thick wide lips are often repulsive because they suggest the African, if for no other reason. But I suspect that the thick lips are also a defect because they are in themselves a hindrance to efficient speech,

and more vitally, because they connote an inefficient formation of the mouth, palate and glottis. Yet it is necessary here again to point out that any of these details may be faulty in a particular case, and yet the others be so well adapted that they more than compensate; and that there may be in many cases language, efficient for thinking, but inefficient for communication. Here as everywhere, our beauty judgments are based on conditions which are general, and to which there are many sharp exceptions.

As regards the teeth, we are in no serious doubt. The beautiful teeth are the sound, regular weapons, which by their form and color give unmistakable evidence of being efficient for chewing as well as for primitive methods of warfare.

While the practical indications of the mouth are important, as I have pointed out, we should by no means overlook the probability of a sexual significance to the evaluation of which the consideration of other beauty characters will rapidly drive us. I need not remind you that popular theory as passed from mouth to mouth and as embodied in literature of all ages, considers both the mouth and the nose as practical indexes of the sex-organs; I should like to express the opinion that popular theory, even popular superstition, is the smoke which always indicates some fire. This particular popular belief is one on which it seems to me it would be worth while for directors of physical culture to make statistical observations.

I need not point out the sexual function of the olfactory organ in the nose of the lower animal; but I ought to warn you against the fallacious opinion that in the human animal the nose has universally lost that function. On the contrary, in a large proportion of the species that function has become more complex. I may add also, that in addition to the significant fact that the membrane lining a large part of the nasal cavities is erectile tissue, there are definite psychological observations (none published, I believe) which throw experimental light on the sexual relations of the nose.

That both the eyes and ears are beauty marks, and that,

in the female especially, they have been selected for especial emphasis by lovers and poets, you are well aware. Both love and poetizing, as most of us well know from our own experience, are conditions of irresponsibility in which the fundamental instincts and habits have large sway; and the first condition usually brings on the second; accordingly the beauty-points which fix the attention of poets demand our attention. But there is little to offer at present in the way of analysis of these. Aside from the indication of physical condition which the eyes afford (and every physician makes use of these indications), the importance of the eye is probably largely racial. The blue or the black, the large or the small, are not in themselves of moment, but they indicate stocks from which we expect certain other characters, mental and physical. The same general consideration is probably involved in ear preferences. This is however by no means the whole story. Anyone who has studied the religious and art symbolism of primitive peoples, and of people not so primitive (I do not refer to the crude and artificial studies of the Freudians) cannot help but see very definite reasons for the fascination of the eye and ear, reasons which are more appropriately discussed amongst psychologists than before a general audience.

Before passing on to the next topic, I wish to protect myself from possible misapprehension by disclaiming any taint of phrenology or blackfordism in the preceding discussion. The significance of cranial and facial characters must be worked out on the lines of physiology and genetics; psychologists have no sympathy with the various systems of so-called character analysis which attempt to decide from a casual examination of an individual what his intellectual and moral peculiarities are in detail.

4. *Hair*. The hair which adorns the human body (or disfigures it, as the case may be), is of two sorts, in regard to its physiological conditions and significance, as well as to its regional distribution. The hair of the head, or *pate-hair*, is the one sort, and the body-hair, including the face-hair, is the other.

The conditions which govern the growth of the pate-hair are not definitely known, but are probably connected with bodily changes which have other important effects. That is to say, the stimulation of the growth of the hair, or the failure of its vitality, are probably due to changes in the internal secretions (hormones) of the organism, although it is not known which of the secretions are the important ones in this connection. It is probable that another effect of the internal changes which produce baldness is a lessening of the resistance of the organism; that the baldheaded man cannot stand the muscular exertion or the nervous strain of which the hairy-headed man is capable. At any rate, baldness is a fatal bar to beauty, both in the male and the female, although to many persons (men especially) an individual of the opposite sex whose pate-hair is exceptionally abundant is repulsive. Another indication of the dependence of the pate-hair on metabolism in other regions is found in the apparent connection between hair and temperament. It is difficult to conceive of a baldheaded musical genius or artist; although even to the rule implied here, exceptions do occur. Temperament, and all emotional factors, as we now know, depend largely on the bodily metabolism, especially on the functions of the internally secreting glands. The quantitative hair character, therefore, may in all probability be reduced to an indication of physical vigor; and physical vigor is far more important, as a beauty asset, than mental ability. Whether the popular belief that the mental ability of a child is in the inverse proportion to the growth of his hair, has any foundation, and whether a similar rule holds for adults, I shall not discuss, as I might be accused of being prejudiced.

The other details of the pate-hair character: fineness or coarseness, straightness or kinkiness, color and contour of distribution, are largely important as indicators of race or stock; yet fineness has a direct sex value in its greater pleasingness to touch. It may also be true that color has a direct value; that the masculine preference for red-haired women which is so frequent, and of which the Elizabethan and pre-Elizabethan erotic writings are so full, is not due solely to the

association of the hair color with the ardent temperament which without doubt was a characteristic of the red-haired stocks; but is in part at least due to the direct effect of the visual stimulation.

All parts of the body except the palms of the hands and the soles of the feet, and certain other small areas, are covered with fine hair, which in the pre-adolescent person are usually so fine and so colorless that they are hardly noticeable. With the beginning of puberty, the axillary hair (the hair of the arm pits), and the hair of the pubic region in both sexes begins to develop, increasing in diameter as well as in length and in pigmentation. In the male also, but slightly later, the face hair undergoes similar development, and still later the hair on the chest, abdomen, and limbs of the male develops in manners which differ greatly in different individuals. In the typical, functionally perfect woman, on the other hand, the body-hair, except in the restricted regions mentioned, remains as fine and as colorless as in the child.

This hair development is not associated with sexual ripening in a chance way, but is controlled by the fundamental sex glands. These glands not only produce the germ cells (the egg and the spermatozoön) whose union creates the life of a new individual; they secrete also, into the blood stream, hormones, *i. e.*, substances which profoundly influence the growth of various parts of the organism. The internal secretions of the male glands produce those changes in the vocal organs which are indicated by the voice becoming heavier and lower; stimulate the growth of the body-hair in the manner above indicated; and undoubtedly produce those structural and functional changes which are evidenced in the tendencies of feeling and action distinctive of the male. If the glands are removed in infancy, these changes do not occur. The secretions of the ovaries, on the other hand, seem to inhibit the growth of body-hair, to accelerate those structural changes in the muscles, glands and skeleton which differentiate the woman from the man, and produce those functional modifications which make the feelings and emotions of each sex a sealed book to the other.

It may be said of the important races of mankind that, in general, the development of the face- and body-hair in the male, and the absence thereof in the female (except in the three limited areas), are alike an indication of fitness for parenthood. The decline of the sex function in old age is usually marked by significant changes in these details. There are of course many apparently anomalous cases, some of which may be explained by glandular details into which the limitations of time forbid us to go; but in spite of these cases, the social verdict is uniform. The hairlessness of the female face and body, and the hairiness of the male face (or the evidence that the hair grows, although shaved off) are important elements of beauty. The male body-hair has little value, because of its irregularity, and the fact of its usual concealment.

There are a number of interesting problems which arise in connection with the body-hair. Theoretically, the pubic hair should be as beautiful, at least, as the pate-hair; yet the Greeks, who set our official standards, did not think so.[1] As to axillary hair, there is lacking information as to its indicatory value. It is an interesting observation, however, and one of no little psychological importance that in these days when the morbid shame of the body is somewhat lessened, and young women expose their arm-pits freely in the ball room and theater, some remove the axillary hair, and others do not.[2] Probably the conflict of opinion in these matters is really between the man's judgment of beauty and the woman's. But we must pass over these details, and hurry on with our main problem.

It is evident now that whether there are other considerations or not, the most important element in the beauty of any individual is the evidence of her (or his) fitness for the function of procreating healthy children of the highest type of efficiency, according to the standards of the race; and

[1] I am informed by Professor Robinson that the Greek women uniformly removed the pubic hair (usually by singeing), probably on account of pediculi. That the esthetic standard is a result of this practice is plausible.

[2] This statement was true at the time at which it was written. Now, a year later, axillary hair is seldom seen.

ability to protect these children. The positive beauty characters we have already examined are clearly such marks of ability to perpetuate the species in the finest and noblest way, and the characters we shall now consider strengthen the interpretation.

5. *Fat.* Here again there are racial differences, but amongst the European races, no racial indications. We may leave out of consideration the Africans and the South Sea Islanders, with their criteria of beauty-fat which seem so odd to us, but which are quite intelligible when viewed in the light of racial characters, and consider Western conditions and standards.

A certain amount of fatty tissue is normal, and is essential for the health of the individual. Fat constitutes a store of reserve material, which may be drawn on in time of unusual need; and without it endurance is limited. This reserve store is probably not so important at present as it was in primitive times, when man lived in a hand-to-mouth way, uncertain today what the food supply would be day after tomorrow. On the other hand, beyond a certain amount, fat is an encumbrance, impeding the operation of many organs, and thus limiting the efficiency of the individual, and also is in itself a symptom of faulty organic functioning of some kind. We are not surprised therefore to find that beauty demands just the right degree of leanness; just the degree which is found in the most vigorous individual.

The standards are somewhat different for the two sexes, because the anatomical conditions and physiological necessities are different. In the female, especially in the young female, there is a special layer of fatty tissue underlying the skin, which is absent in the male. This gives her the roundness and softness of outline which is essential to the perfection of feminine beauty, and also prevents her from feeling the cold so much as the male does. Possibly also it explains why she swims more easily. (It is a fact that women are as a class far better swimmers; this has been ascribed to the better development of the legs· but this reason is hardly sufficient, since it has been shown that leg action is the least important factor in swimming.)

The softness and roundness of contour of the female is beautiful, because it is the mark of physical fitness. The fatty layer is supposed to be an extra reserve supply of food material, laid up against the heavy demands which are made by child-bearing, and in still another way protects her in that supreme process, of whose splendid fruition beauty is the glorious blossom. When age withers, through the absorption of the adipose tissue, primary beauty is on the decline, and unless it be replaced by the secondary beauty appropriate to advancing years, the drama of life becomes a tragedy. And indeed, the great fact that we all must face at some time, that the strength and vigor of our prime is past, and that the time when the almond tree shall flourish and the grasshopper become a burden advances upon us, is usually announced to a woman in the discovery of wrinkles due to the slipping from her of her subcutaneous robe of office.

6. *Complexion.* The tint of the skin, of course, is largely a racial indication, but in certain respects, the tint, as well as the texture, is an index of health and vigor. The standard of beauty in complexion, whether light or dark, is that which goes with the full bloom of sexual vigor, when the human organism is at its perfect development for the perpetuation of the species. This is so obvious that it would be superfluous to discuss it further.

7. *Muscular tonicity.* The voluntary muscles of the body, *i. e.*, the muscles of the face, scalp, trunk, arms and legs, are kept in a condition of *tonus*, by nerve currents constantly supplied to them by the motor nerves. Tonus is a state of partial contraction, which constitutes the readiness for action of the muscle. If the motor nerve trunk which supplies any voluntary muscle be severed, the muscle at once becomes flabby. The tonus does not depend entirely on the nerves which stimulate the muscle. In order to be stimulated, the muscle must be in the appropriate chemical condition to receive the stimulus, and this chemical condition is dependent not only on the general metabolic conditions of nutrition, fatigue and rest, but also on the specific actions of hormones produced by several of the internally secreting glands, notably the adrenalin produced by the adrenal glands.

In case of injury or disease affecting certain parts of the nervous system, certain muscles become flabby. In case of general flabbiness, it is of course not evident immediately whether the primary defect is in the nervous system, or in the metabolism of the body. In any case, flabbiness, local or general, is a symptom of inefficiency in bodily functioning, and although under modern conditions the flabby individual may be able to make his living at his particular restricted occupation, flabbiness unfits him for parenthood now, just as much as it did in the stone age. We can't breed husky children from flabby parents.

The flabbiness which is due not to a specific injury or disease, but to insufficient vitality, is first shown by the muscles of the face. That is to say, it is first shown to the casual observer; a medical examination would probably find it in other muscles first. It is not entirely due to the concealing of the body that the facial muscles have become known as the muscles of expression. Failures of tonicity in these muscles are conspicuous; the sagging eyelids or corners of the mouth, or cheek muscles and other modifications which are readily observed but described with difficulty, are common traits which are fatal to beauty. In fact I do not hesitate to say that assuming the conformation of the features, and the complexion, to be not actually objectionable (that is, assuming the bare negative conditions), beauty, in so far as it is facial, depends on the proper tonicity of the muscles.

The *activity* of the facial muscles expresses the mental and still more the emotional *activity* of the individual in a plain way. Vivacity and dullness, cheerfulness and gloom, benevolence and rancor, interest and ennui, and a multitude of other conditions are written in the facial movements for the runner to read. Boldness, modesty, candor, deceit, innocence, guilt, and other moral qualities may be expressed in the contractions of the muscles surrounding the eyes. But in repose, these muscles are expressive in another, and perhaps more important way, for they show the potentialities of the individual; what he is capable of, in so far as the capability

depends on the functioning of the nervous system and the endocrine glands. A person may be attractive, while the face is in action, because the action indicates a desirable type of mental or moral activity going on; but she is not to be judged beautiful in face, unless the face in repose expresses desirable potentialities. A common form of expression is "she is beautiful only when she smiles": a better statement would be "she is attractive when she smiles, but she is not beautiful."

Although our survey is far from complete, it has proceeded far enough to show us clearly in what beauty consists. It is the sign and the expression of the *potentiality* of the individual; not what he has done or is doing, but what he is capable of doing; not what he is capable of doing for his own interests, but what he is capable of doing *for the species.* Put in the plainest of terms, the most beautiful woman, the handsomest man, are the persons we would choose to be co-parents of our children, if we considered nothing but the highest mental and physical welfare of these children.

The reasons for the actual matrimonial choices of society are complex, beauty being only a minor consideration. For the student of social psychology the investigation of the other factors is of absorbing interest, but here I may say merely that the predominance of these factors is a calamity. As a physiological psychologist, I must repeat what the poets have sung: the glorification of beauty and its exaltation as the primary ideal, which ought to reign in human life. Of all the divinities in the Greek pantheon, the most glorious are not Zeus and Hera, not Ares and his Aphrodite Pandemus, but Apollo and Aphrodite Urania, the life-giving queen of heaven.

It will be noticed that I have omitted moral qualities from the composition of the beautiful individual and have ignored the physical characters which connote these qualities. In this I have been consistent, and am in perfect agreement with common usage. Beauty may be proud, cruel, deceitful, immoral, wicked; and yet it may still be beauty. Cleopatra was capable of almost any crime you can think of, and Thaïs

was no modest violet; but history tells us that they were of wonderful beauty. 'Handsome is as handsome does' is true only in a qualified way.

How, then, can we elevate beauty to the rank we give it, since it satisfies our social demands only in part, and in what many consider the less essential part? We must do so, because it is the foundation on which truth and holiness are built. Only the race which is physically and mentally fit can survive and flourish long enough to develop and put in practice moral ideals. The problem after all is not one of choice between two ideals, but of having such regard for the primary ideal that it may help us to the attainment of ultimate ideals. In a more specific and limited way the problem of right and might exemplifies the guiding principle, which is therein not a choice *between* right and might, but the bringing of might into the service of right.

So much for the salient characters of beauty in the meager treatment I can give them here. I might now mention two other points which possibly will set off more clearly the conception I am trying to express.

Although beauty, in the primary and fundamental sense of the term, is prospective, we sometimes use the word retrospectively, as when we speak of a beautiful old lady or a handsome old man, indicating thereby a person who evidences the past possession of characters valuable to the race. In a certain sense, the retrospective characters of beauty are the same as those which constitute beauty proper; but nevertheless there is a tendency to admit, or rather demand, especially of women, moral characters not demanded in the case of primary beauty. While the handsome old man is rather strictly the man who still retains in some degree the marks of positive beauty (the marks having a retrospective significance), the beautiful old woman is she who, retaining the retrospective characters, also gives evidence of graces and temperamental qualities which are possibly more the result of environment than of constitution, and which in the younger woman are set off from beauty as "sweetness."

This admission of retrospective personal values is one

feature of the consideration which civilization has given to the aged, *i. e.*, to the individual no longer potential for the race. This consideration, perhaps, has not increased since patriarchal times, but it is an advance over the attitude of still more primitive races amongst whom the individual who is no longer useful as a warrior or a parent is ignored or eliminated.

Finally, I must refer to the popular distinction between prettiness and beauty; a distinction which at least as it applies to women rests on solid psychobiological grounds, and which offers abundant opportunity for psychological research, having practical application to some of the pressing social problems.

The pretty woman is she who possesses certain of the characters of beauty, but in such combination that they are not an indication of the general potentiality requisite for beauty. The characters of prettiness are the characters of beauty which promise least for the stamina of the race. Without extensive analysis of these signs the distinction may be summed up by saying that a pretty woman might be the man's choice for a mate, but not, other considerations being subordinated, for the mother of his children.

There is doubtless a valid distinction in types of men, corresponding to the distinction between 'beautiful' and 'pretty' women, but it is practically unimportant because of the singleness of woman's judgment. Men, however, are as a sex strongly interested in *pretty* women as well as in *beautiful* ones.

On this point, certain observations on theatrical performances, especially musical comedies, are illuminating. Details are too lengthy to introduce here; but in brief, the types represented by the show girl and the dancers are necessary to give the chorus (the foundation of the show) the widest appeal to the males. This is a fact of practical importance to producers, and I have found no difficulty in obtaining abundant introspective confirmation from men of all classes. Some men are interested almost exclusively in the type of show girls who evidently would be splendid mothers; others

are primarily interested in the types who are attractive in a more immediately sexual way. The great majority of men, however, are strongly interested in both types, and have little difficulty in identifying the grounds of the two interests. The stage, I may remark, is to social psychology what the laboratory is to individual psychology, furnishing the possibility of experimental tests, especially in the domain of the problems of the *family*, to which the topic of this paper properly pertains.

I have sketched, in the preceding discussion, the line of observation and reasoning which supports my opening statement that beauty is something vitally important for the human race. It is unnecessary that I should fill in this outline with more detail, because, having once become impressed with the scheme, whether favorably or adversely, the details will be filled in from your daily experience, and will in the end leave no doubts as to the truth of the matter. It is therefore the business of the social psychologist to lead the way from this point to the next, and practical one, the *conservation of beauty*.

THE FUNCTIONING OF IDEAS IN SOCIAL GROUPS

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In social psychology terms are still commonly used in a very loose and vague manner. It is not so much the kind of terms used as it is the conception of them that is to be guarded. The child inherits socially from the group his ideas, language, customs, etc., and these mold him gradually into the likeness of his fellows, roughly speaking. It would seem desirable in view of certain tendencies in psychology to attempt a more precise statement, in terms of *stimulus and response*, of the social relationships of individuals.

The explanation of behavior in terms of ideas is liable, for several reasons, to fall into a vagueness which it is desirable to avoid; nevertheless it does not seem to the writer that it is well to abandon the use of the term *idea*. The mere adoption of new terms does not meet the difficulty; it does not supply the analysis needed. There are other important reasons for retaining the term. Writers in social sciences will continue to use terms intelligible to the non-technical mind, terms, moreover, which imply that man is a conscious being, imagining and reacting ideationally to environment not directly present, or present only by some sort of representation and not *in toto;* and it is the business of psychology to analyze and explain the various responses of man to his human environment so that the results of such studies may be available to workers in these social sciences. In this obligation psychology seems to have failed in the past. This failure has usually been attributed to a too academic and structural analysis of merely the 'content of consciousness' without satisfactory relationships to the environment. Some psychologists have regarded this attitude and procedure as an essential preparation to the more functional studies, one that is necessary to assure greater scientific accuracy in

subsequent applications of psychology. This contention does not seem to have been supported by the results and many psychologists are frankly striking out along different lines.[1] These present tendencies do not disprove that there may be a legitimate analytic interest in the conscious life of one's self *per se;* but science requires that there be some sort of objectification of results, and it also has in view, implicitly or explicitly, the control of human environment for man's own ends. It is, of course, fair to say that in evaluating the results of any scientific tendencies or methods, or points of view, the question is not usually one of *all* or *none*, but one of *how much.* There is no question that any sort of unbiased analysis of the mental processes results in freeing man from various superstitions and in giving him a better understanding of his social interrelations. Results, moreover, must not be judged too immediately, or from too narrow a criterion as to what is 'scientific.' Certainly one cannot but be appalled at our present ignorance of human nature and behavior in most of its larger aspects.

The current question of behaviorism versus introspection[2] will no doubt finally resolve itself into one of better control of all the stimulating conditions in experimental problems. If it be urged that this has been the aim of the introspectionists for a long time, and the occasion for experiment, the reply is that the usual introspective studies have paid attention only to certain more immediate aspects of the conditions of the experiment and have failed to recognize others, notably instinctive tendencies, unconsciously acquired attitudes, and the larger orientations and motives of the subjects. Moreover, the reactions of the subjects have been defined too narrowly and too immediately in terms of consciousness, and such terms may or they may not (generally not) have common significance to all the investigators concerned. *Terms are, after all, objective matters themselves, and not subjective content.* The same use of terms by two individuals does not

[1] Cf. McDougall, 'Social Psychology,' Ch. I.

[2] *E. g.*, Watson, J. B., 'Psychology as the Behaviorist Views It,' Psychol. Rev., 1913, **20**, 158–177. Angell, J. R., 'Behavior as a Category in Psychology,' *ibid.*, 255–270.

guarantee in itself common meanings, much less similar 'conscious content.' The question, then, finally reduces itself in any event to one of mutual agreement in the designation of various subjective content, on the one hand, and of behavior, on the other. In either case we have reactions to very highly abstracted objective stimuli, *i. e.*, to words.

This brings us more directly to the subject of our thesis. The objection to the too loose use of the term 'idea' in social psychology[1] is on this same ground; it is based on a lack of agreement as to what 'idea' implies in its genesis and in its *modus operandi*. What constitutes an idea, and how does an idea modify behavior? It is frequently said in scientific literature that if one has an idea of an act, with no contradictory ideas, the idea must express itself forthwith in the act.[2] Thorndike[3] has made an extensive and somewhat academic argument against this doctrine, with some 'experiments' to test its validity. It seems to the writer that he has mainly missed the mark so far as usefulness to psychology is concerned. His intention in the investigation does not seem to have been wholly serious; he doubtless regarded the experiment rather as an after-dinner diversion for the psychologists in their association meeting where the paper was read, and intended, more seriously, to show thereby the need of a different viewpoint.

An idea may be regarded as an acquired disposition to behave (explicitly or otherwise) in a certain manner on the occurrence of a given stimulus. The stimulus bringing about such a response may be direct or indirect, representative; and it is always more or less complex, due to associations with or inhibitions from any combination of the scores of

[1] This is an example taken from a well-known writer. ". . . ideas are active factors in the social life, because ideas, in civilized man at least, come in time to constitute for the individual and society a sort of 'subjective environment,' as Professor Ward admits, and this subjective environment the mass of individuals respond to quite as they do to the stimuli in the objective environment. In other words, ideas modify activities directly without the interposition of feeling, just as sensation and images coming from stimuli in the objective environment call forth responses." Ellwood, 'Sociology in Its Psychological Aspects,' p. 265.

[2] *E. g.*, James, 'Principles of Psychology,' Vol. II., 522 ff.

[3] Thorndike, E. L., 'Ideo-Motor Action,' Psychol. Rev., 1913, **20**, 91–106.

stimuli (both internal and external, intra-organic or extra-organic) that constantly play upon the organism in changing relative degrees. Because of changes in time relations and in intra-organic conditions it is utterly impossible that a precisely identical stimulating condition can be presented twice in succession to an individual. This is true, of course, even of a written word as stimulus. We are beginning to know how profoundly the organism is altered, for example, in emotional attitudes. The same thing is doubtless true, though in a more subtle fashion, of the various more cognitive attitudes. The entire *neural set* at any one time is determined by numerous uncontrollable conditions. This is particularly noticeable in various cases that have been described in pathological literature, as in tendencies to the dissociation of personality. The effects on any immediate stimulus of more general bodily attitudes is strikingly evident in such illusions as disorientation of direction with respect to the cardinal points,[1] also in less permanent illusions. For example, in walking among the numerous small bodies of water known as Lake Minnetonka, Minnesota, one is frequently surprised to see a small 'lake,' when first visible, at a much lower level[2] than anticipated and to note that the surface of the water has a considerable decline toward the opposite shore, or away from the observer. This illusion results when one unawares walks up a gradual incline from one lake toward the next. In animal behavior it has been found that an animal reacts in maze experiments not simply to the particular bifurcations present to it at any given time but rather to the whole maze which it has previous y traversed.[3]

Nevertheless a person usually attends to, and is therefore mostly affected by, certain selected stimuli; hence the recurrence of certain symbolic stimuli, like written or spoken words,

[1] Peterson, Jos., 'Illusions of Direction Orientation,' *J. of Phil., Psychol. and Scientific Methods*, 1916, 13, 225–236.

[2] The 'lakes' all have the same level.

[3] Hubbert, H. B., and Lashley, K. S., 'Retroactive Association and the Elimination of Errors in the Maze,' *J. of Animal Behav.*, 1917, 7, 130–138. Peterson, Jos., 'The Effect of Length of Blind Alleys on Maze Learning: An Experiment on Twenty-Four White Rats,' *Behav. Monog.*, 1917, 4 (No. 15), pp. 53.

normally brings about a high degree of uniformity in the responses of the same individual and of different individuals of approximately the same environment. Ordinarily we say they have *common meanings*, but it is obvious that on the last analysis this can be ascertained only on the basis of similarities in response of some kind.

The response to words—with a few important exceptions in which instincts are aroused by certain sounds—are wholly acquired by associations. But, as has already been implied in the previous paragraph, mere passive association is not the whole factor. The organism has certain innate selective dispositions and association serves only to direct in a measure the basic organic activities which are motivated largely from within, as is the case with hunger, restlessness, excitability to moving things, etc. For example the writer tried to give a child nearly a year of age the idea 'moon.' The child was taken out of doors on an evening when the moon was full and nearly overhead. Attempts failed at first to get the child to attend to the moon, though the latter was pointed at persistently as the word was spoken. At this juncture a street car passed. The child's attention was attracted by the moving object with its brilliant lights. To the word 'light' which I spoke while pointing toward the car, the child responded by likewise pointing and saying 'ite.' When the car had passed similar responses were evoked to certain lights in the distance, and finally to the moon. The term 'moon' was now substituted, the child responding and attempting to say the word. After a number of repetitions of this kind the child would suddenly look up and point to the moon whenever the word was spoken by me. Afterwards when the child was in the house he would crawl to the window or door and look up for the moon, repeating 'moo! moo!' Finally on my appearance later he would frequently take the initiative in saying the word and in pointing out of the window at the sky. When taken out he would look all around for the moon. He had 'got the idea'; not, of course, exactly the idea that I had, but one—*i. e.*, a kind of response—consistent with his own more limited experience. Whether

or not he had the same imagery—visual, auditory, kinesthetic, etc.—that I had, or the same that any one of us would have had, is not important here. There is every reason to doubt such community of imagery in adults, to say nothing of community of affective states. Do two adults ever have precisely the same idea? This can be answered, if at all, only from their reactions, verbal or more overt acts, and depends in a large measure, too, on just the precise meaning given to 'idea.' For the present we have no interest in a mere academic discussion of this question; but for our practical purposes we may conclude that people have the same idea when they react overtly alike in all the essential details to the associated stimulus.

One is likely on safe grounds in holding that all ideas arise by a process in the main like that just illustrated in the case of the child, that is, that ideas are developed by the association of certain acts with given stimuli. But this association depends, as was suggested in the foregoing, upon inner instinctive tendencies and selective dispositions. It is of course admitted that when many ideas have been acquired by a child he may elaborate these and develop new ones by various combinations of those already attained, and by the substitution of one term already understood for another. That ideas come thus from experience wholly no one would likely now deny, though it could not be held by anyone that the mind begins with the passivity implied by Locke's *tabula rasa* conception. It is also to be noted here, to avoid misunderstanding, that in experienced individuals many of the responses here spoken of are not immediate overt acts. Many are more or less delayed, being impeded by others, sublimated into larger responses to almost inconceivable complexities. Allowing for such complications and indirect responses—provided that the instinctive dispositions of the subject are normal and that the fatigue effects and other physiological conditions are as before when the idea was acquired—I see no reason why an idea should not express itself in an act when the recurring stimulus 'brings it up in the mind.' Of course Thorndike's experiment failed to take note of these

other factors, and the words presented visually to his subjects were presumed to be the only stimuli operative! This is precisely the error that has persisted in the usual statement by writers in social psychology, that the child takes on the ideas of the race by suggestion, imitation, and so on.

Our point is that to make social psychology free from some of these errors we must get objectively at the nature of ideas, and must understand their origin. From this point of view an idea is a disposition to response, which is associated with a certain kind of objective stimulus, usually a spoken or a written word. Physiologically it is a stimulus-response mechanism whose neural connections are empirically established or developed, a mechanism which in the highly developed individual (or 'mind') becomes extensively particularized both as to the type of adequate stimulus and as to the nature of the response; *i. e.*, it becomes *freed* from other stimuli and responses usually associated with it in a larger complex.[1] This does not, of course, deny that in larger and more complex stimulus situations it may function in combination with other such mechanisms. In this sense comparative psychologists speak of 'free ideas,' which are usually denied to animals below man. An idea, then, is a sort of acquired, more or less detachable, stimulus-response disposition, or habit. In its most elementary aspect it may function in numerous combinations with other such habits, just as a given word may function in various contexts in a language.

Strictly speaking nothing is communicated when one person tells another something. Not even *meanings* are communicated, in this sense. The second person is stimulated auditorily—or visually if he reads the communication—and what he gets out of the stimulus is what his own experience or training has associated with the word-stimuli. This is obvious if persons of different language groups attempt to converse. They do not understand each other, and must resort to gestures that have approximately equal associations for the two individuals. They have not become accustomed

[1] In the reference given in the preceding footnote the writer has developed this idea more fully.

to the same word associations. Conversation in highly abstract and condensed terms is possible only to those with similar experience sufficient to make quickly the multiplicity of associations with previous acts or experience suggested. That is why education must always, if it is to be real and function successfully, keep close at first to laboratory experiments and to actual first-hand experience, and relax only gradually on these matters as the individual gets the necessary basis for the more symbolic procedure.

Why is it then that ideas are so persistent in generations, and that the older individuals get the new ideas with so much difficulty, if at all? Why is it that the social world changes so slowly? This is most easily understood if we think of the fact that for any important change to take place in society *both the stimulus-systems and the acquired habit-systems must change together*. Wholly new stimuli have no significance; they do not call out any definite response. New ideas must be *learned* just as more explicit acts or habits are learned by an interaction process. Habits are changed only as the necessities of organic needs demand, or as compelled by circumstances. Moreover, and of particular significance here, it is to be noted that, since all the members of the social group have acquired practically identical responses to the several stimuli, one person cannot begin new types of response to particular stimuli without conflict with his fellows. It is also impossible to change instantaneously and extensively the usual stimulating conditions. These difficulties are met, *e. g.*, in our attempts to simplify our spelling, or to get rid of our Fahrenheit thermometer scale. We cannot get rid of old books at once; hence our children must, on the one hand, be under a confusion resulting from the use of both kinds of spelling with the old predominating, and the adults, on the other hand, who have learned to spell in the old way, cannot easily fall in with the new. Only a few enthusiastic individuals, who have been influenced indirectly by certain scientific tendencies, act as the new stimuli to other individuals. Likewise with the thermometer situation and with the taking on of new kinds of tools and of improved methods.

The old materials cannot be at once destroyed without too much waste and effort, nor can the new habits be suddenly acquired by all. The new meets difficulties both of a physical and of a physiological nature. This dual aspect of change in social customs, beliefs, etc., has usually been neglected because of a too subjective treatment of ideas. It is instructive to imagine the inconceivably complex modifications required in any change of this kind. These difficulties are all obvious and concrete when we regard ideas objectively as acquired tendencies to respond in certain ways on the occurrence of given stimuli. This view does not, of course, deny in any sense the subjective aspect of ideas, imagery of various sorts, or their accompanying feeling states.

Looked at from this standpoint the so-called laws of suggestion become more intelligible—some of them less intelligible, if for intelligibility they must be true! A restatement of our suggestion psychology from this standpoint is most desirable. Some of its extravagances would be bound to disappear. The same thing is doubly true of that marvelous instrument of modern 'research,' the *sub-conscious*, or *co-conscious*.[1]

It is to be noted, then, that ideas are acquired by any social group in accordance with the stimuli which the group encounters, such, for example, as printed matter, spoken language, and various physical objects and symbols of significance. These ideas in turn, these habitual types of response, modify and further elaborate the various symbols and objective environment; and so a process of give and take between the individuals and the objective, standardized symbols develops into a civilization, a 'race inheritance.' Civilization is not only a matter of habits and ideas, but also just as much a matter of inconceivably complex organization of objective environment. The latter is intricately interwoven with the former both as cause and as effect. Each individual, moreover, is objective environment to every other;[2] and as different individuals overlap variously in their years of

[1] The two terms are not exactly synonymous, as currently used.

[2] This view is not incompatible with Cooley's view of society so penetratingly analyzed in his 'Human Nature and the Social Order.'

existence, some of all ages existing simultaneously, changes in ideas, customs, and conventions are difficult. Big crises bring changes about most easily *because they affect all individuals somewhat alike at the same time.* Thus a great war threatening a social group or a state may bring about important changes with surprising rapidity, as is seen in the present world conflict.

Giddings's 'consciousness of kind,'[1] as well as other subjective statements of perfectly obvious facts about society, is easily and more objectively and concretely explicable on this view. An individual reacts easily and with little friction in familiar environment, *i. e.*, to the interrelated groups of stimuli to which he is accustomed or by which he has been molded; but as he gets away from these stimuli, even though they are not wholly different from those in his own group, complications arise in his responses. This is true both of his overt acts and of his intra-organic processes. His acts become disorganized, uncoördinated, and aimless. Often they are inefficient to a high degree, while, subjectively, feelings of lonesomeness, of exaggerated self-consciousness and of discouragement arise, reflecting inner disturbances. Technically speaking it is not these *feelings* that return one to the group, nor the *consciousness of kind* that keeps one there. Such is only a superficial and popular view of the matter, one that results in vagueness of explanation and description and that renders inadequate means of social control and social modification. The difficulties of functioning and of general orientation in foreign social groups, with their new combinations of stimuli and the absence of old ones to which habits have been adjusted, are the important things to consider. The situation is such as to favor reaction back to the old familiar conditions. Imagine a person going into a foreign country where language, customs, the medium of exchange (money), etc., are strange. He not only fails in numerous ways to make the adjustments necessary for his own bodily and social needs, but he also thereby becomes marked as different from other individuals, as peculiar, and

[1] Giddings, F. H., 'Inductive Sociology,' 1901, 94 ff.

may bring down upon himself all the protective hostile reactions of the foreign group. Even a strange accent may do this; or, among groups speaking the same language, numerous provincialisms, habits, and evaluating responses, in which each individual is typical of the community that fashioned him, may serve as shibboleths. Preliminary studies of group-differences based on word-association reactions[1] and on certain intelligence tests[2] are about all that have been scientifically made by psychology in this field. In comparative psychology some studies have been made, in many cases more or less incidentally, on the reactions of certain mammals[3] and insects[4] to individuals of other groups chiefly on the basis of characteristic odors. The whole field suggested in this paragraph is practically untouched and is full of possibilities for empirical psychology.

Imitation and suggestion are no longer adequate terms by which to explain how a child takes on the language, customs, beliefs, etc., of the group into which he is born. In the first place such explanations imply too great passivity on the part of the individual. The child acts instinctively in numerous ways from the time of birth, or, more precisely, inner stimulating conditions and bodily needs bring about various acts which persist until the stimulating conditions are changed. The instinctive impulses are modified by a trial and error process and become adapted to the environment into which the individual is born. Many of them come into conflict with the acts of other persons. Impediments and inhibitions result in numerous more or less subtle ways, and as the child grows his acts naturally find expression and organization along lines of greatest consistency, or of least opposition and obstruction. Thus society results by means

[1] Woodrow, H., and Lowell, Frances, 'Children's Association Frequency Tables,' Psychol. Monog., 1916, No. 97. Group differences are only incidental features of the monograph.

[2] Several of the Binet-Simon tests in America have incidentally shown differences among various social groups.

[3] *E. g.*, Watson, in 'Animal Education,' 1903, found evidence of what he supposed to be reactions to sex odors, but which may as plausibly be interpreted, so far as his experiments go, as reactions to group differences on other than sexual basis.

[4] See Washburn, 'The Animal Mind,' 1917, 100 ff.

of the suppression through conflict of all habits and tendencies not suited to coöperation, and by the facilitation of acts mutually beneficial and objectively consistent.[1] It is obvious, *e. g.*, that in the acquirement of language and ideas by the child these factors play a larger rôle than usually supposed, and that passive imitation is not the important factor that it has been made out to be by such writers as J. M. Baldwin. Moreover, experiments on learning have shown, as is now well known, that new acts must be brought under control of proprio- and intero-ceptive stimuli by a random trial and error procedure, in which the fittest acts survive, before they can be called out so simply by the sight of others' acts. In the subjective terminology, one must get the resident (kinesthetic) imagery of new acts by trial and error and associate it with remote imagery before the acts can be controlled sufficiently for imitation. Even then imitation must have some adequate motivation.

The Freudian writers are right in putting their emphasis on instinctive and intra-organically initiated activity with consequent conflicts, repressions, sublimations, etc., whatever objection one may have to their over-emphasis of the sexual impulses and to such special explanatory machinery as subconscious 'censors.' Habits, as McDougall has insisted,[2] are formed in the service of instincts. The instinctive dispositions do not change and function differently just

[1] On this basis the repressions of the Freudians may receive a reasonable explanation. In an article entitled, 'Completeness of Response as an Explanation Principle in Learning,' Psychol. Rev., 1916, 23, 153–162, the writer has developed the view expressed above in its application to learning. Experimental support of the view is given in the monograph referred to in note 3 on page 217. In experiments with animals he has shown that frequency and recency laws actually do not explain the rat's learning in the maze, 'Frequency and Recency Factors in Maze Learning by White Rats,' *J. of Animal Behav.*, 1917, 7, 338–364. The completeness of response principle seems to be applicable to social adaptation. Responses to stimuli in social environment do not take place immediately as a rule, often not for long periods of time; in the meantime other stimuli are received, the effects of which overlap with those of the earlier stimuli. Responses, then, are along the lines most consistent with all the stimuli, or with the entire situation. These are the most complete in the sense outlined in the theory. This is what makes them usually pleasant, the greatest consistency with organic needs and the external situation. Facilitation of the nerve impulses along certain tracts thus arise, and useless acts are gradually eliminated.

[2] 'Social Psychology,' p. 43.

because one sees other individuals perform certain acts; changes in all our habits and ideas come only as they are forced by the maladjustments of one's instinctive equipment, and such changes in turn modify the various symbols by which ideational responses are brought about. Individual innate differences are of course important in these processes of mutual adjustment between individual and environment.

THE ATTRIBUTES OF SOUND[1]

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My special interest in the attributes of sound began, I think, in 1903, when, through association with Professor Max F. Meyer, I became familar with his important work in audition. Up to this time I had accepted the traditional account of the attributes of sensation as being four in number: quality, intensity, extensity, and duration. When applied to sound this view identified pitch with quality, while extensity was represented by a vague concept of volume. Meyer in his paper 'On the Attributes of Sensation'[2] impressed me with the notion that there need be no *a priori* assumption that all sensations should have the same formal attributes. I also learned at this time of Meyer's distinction between *quality* and *pitch*. The former he referred to *height*, in the sense that every sound has its characteristic place in a progression, best described as a variation from mellowness (lowness) to shrillness (highness). A sound may also have pitch, *i. e.*, musical relatedness. In this case it is a *tone*. The attributes of sound, as given by Meyer, were four in number: duration, intensity, quality, and pitch. Extensity disappears. Whatever there may be of voluminosity is absorbed in quality. But Meyer's attributes are not the independently variable aspects of a sensory content. Rather, they are like atoms of consciousness that can be judged or compared with the aid of attention. Noise lacks pitch altogether, for it cannot be used musically. Objection can here be raised because pitch as so described is a *meaning*: it is musical significance, what else it may be does not appear.

Meyer stated in his paper that 'the average psychologist

[1] Paper read before the American Psychological Association at Pittsburgh, Dec. 27, 1917.

[2] Psychol. Rev., 1904, 11, 83–103.

takes . . . little interest in auditory sensation.' This seems to have been true, for Meyer's contribution to the scientific description of sound exerted but slight influence upon psychologists until they were confronted with the more startling results of Köhler, Révész and Jaensch. Then, at last, such an interest began to awaken. Révész[1] in particular discovered a remarkable pathological condition in the hearing of his friend and collaborator v. Liebermann. The latter proved to be unable to judge fusions and musical relations correctly in certain regions of the scale, although his sense of interval-distance was unimpaired. This led Révész to a distinction in the pitch attribute analogous to that which Meyer tells us he got from Stumpf, and made public as early as 1898. But Révész reverses the terms: quality becomes his designation for musical relatedness and fusion, while pitch becomes brightness-height or dullness-depth. In the interest of a uniform terminology, Meyer[2] has recently suggested that the first attribute be called *tonality* and the second *vocality*. But these terms, as we shall see, embrace too much. Besides they are still open to the objection that they incorporate meanings into the structure of sensation.

Watt has now subjected the whole matter to a critical review that culminates in the presentation of a comprehensive theory of the psychology of sound. This he incorporates in a recently published volume.[3]

Watt returns to the traditional classification of the attributes and attempts to establish a uniform list applicable to each of the senses. It is through integrations of these attributes, he believes, that all conscious processes arise. By way of certain uniformities we arrive at the unity of consciousness in which the various sense modalities figure. The attributes he suggests are quality, intensity, 'systemic order,' extensity, temporal order and durance. Quality signifies the essential nature of the sensation, as cold, warm, red, bitter. In the realm of sound there is but one quality,

[1] *Zsch. f. Psychol.*, 1912, 63, 263ff., 325ff.

[2] *Zsch. f. Psychol.*, 1914, 68, 115ff.

[3] H. J. Watt, 'The Psychology of Sound.' Cambridge, The University Press, 1917.

though there are three important integrations: tone, noise and vowel. Intensity requires no special comment as to its meaning. 'Systemic order' underlies all localizations and positions. In sound it is what we call pitch. Extension is the attribute of 'spreadoutness;' in sound this is volume. Temporal order is the basis for comparison of units in time, as evidenced in rhythm, while durance is mere protensity, or the temporal spread of sound.[1]

With regard to tone, Watt urges that this is always a massive phenomenon. The volume of a tone embraces a number of 'systemic orders,' one of which, rising to prominence, marks its pitch. The rise of this predominant order, together with the others included in the volumic whole, measures the tone's intensity. The 'systemic orders' incorporated in a sound are of great importance in determining fusion and sequential integrations. It is in this way that Watt explains musical effects. The predominant order, or pitch, is normally central to the tone's volumic outline. Volumes decrease regularly in size as pitch increases in height. Hence, when two tones in the octave relation are simultaneously sounded, the upper tone will fall entirely within the volumic outline of the lower tone. Furthermore, the order of the higher tone's pitch predominance will be such that it occurs midway between the lower tone's predominance and the upper limiting order of its volumic outline. The upper limiting orders of volume for all tones coincide, because two tones simultaneously heard never stand apart, but always interpenetrate. In the case of the octave "only one natural pattern offers itself as obvious: that in which the extreme order included in the volume of the higher tone on its lower side coincides exactly with the predominant order of the lower tone."[2] On this ingenious foundation Watt constructs his theory of fusion and musical interval as resting upon a balance of the resultant sound mass when one sound coalesces with another. (See Fig. 1, p. 238.)

[1] "To distinguish temporal order from the order upon which localisation rests," writes Watt, "the latter may be called systemic, as it is the order that appears when a system of receptors yielding one quality is given." *L. c.*, p. 9.

[2] *L. c.*, p. 64.

Coming to Watt's interpretation of v. Liebermann's case, we find him suggesting that the pathological condition is in reality a shifting of the pitch predominance away from its normal central position in the volumic mass. Hence, the fusional balance is destroyed, although the volumic outlines remain as usual. V. Liebermann judges his distances correctly. He knows that the octave span is greater than that of the fifth, yet he cannot always detect a fusional difference between them.

Although adequate experimental evidence for Watt's theory is lacking, it must be noted that such results as we have are rather in line with his proposals. Dr. G. J. Rich, for instance, who has experimented on volume discrimination, finds it to follow the geometrical progression of Weber's Law. His recent investigation with pure tones, which I am privileged to cite, indicate the limen to be approximately 6 vib. in the region of 275; 12 in the region of 550, and 24 in the region of 1,100. The fractional increment is thus about .02. This uniformity suggests that the volumes of tones in octaves are halved in size as one proceeds upwards in the scale, which is the conclusion Watt reaches on theoretical grounds.

The difficulty of Watt's theory does not rest on an assumption of volumic coincidence so much as in the 'symmetry' and 'balance' inferred in the case of all fusions. This is his explanation of fusion: that two tones fuse when the volumic pattern of the higher one balances with, or is symmetrically placed within, the volumic pattern of the lower. For such emplacement the decisive factors are the terminal limits of the two volumes, but these limiting orders must be consciously and physiologically negligible. Strictly speaking symmetry is only found in the octave, and a true balance only in the fifth. Other fusions seem to rest upon accommodations resulting from a sense of proportionality with regard to the intervals employed. Note, for instance, the graph of the *fourth* in Fig. 1. The balance is here disturbed because the predominant order of the higher tone is twice as far removed as is its lower limiting order from the predominant order of the lower tone.

My own reaction to this matter is determined by a different point of view. While Watt attempts to overcome the difficulties of a purely structural psychology by working out an elaborate system of integrations among the attributes of sensation, I am disposed to a functional interpretation, in which I accept mental acts as distinct from mental contents. The attributes that constitute a sensation are basic facts upon which the mind operates, but the operations themselves are not inherent in the attributes. Neither fusion nor musical relatedness, I think, can be adequately accounted for without reference to an *attitude*. The attitude is dispositional and embodies tendencies to conscious response: directions and determinations that have their origin apart from the attributive nature of the tones fused or related.

The revision of the attributes of sound that I am about to suggest is based upon this functional point of view. Since I leave much to *cognition* as a function, I do not find it necessary to incorporate among the attributes all that Watt deems requisite. Nor do I find it theoretically important that a uniform set of attributes should be equally applicable to all the sense modalities. Yet mental functions can operate in the first instance only upon what the sensations offer. If mental acts are capable of creating mental contents, as I believe them to be, such contents are of a different order, namely, the thought contents. While thought is qualitatively distinct from sensation, as a complex of attributes within a certain modality, still it is from comparisons instituted among the attributes that these derived contents of thought or meaning come into existence. Hence their creation does not occur in a world apart from that of sensory experience. They are founded in it and upon it.

The attributes of sound as I find them are not primarily determined with a view to system, but are describable facts of consciousness. In this instance my findings have been largely gained from participation as an observer in the experiments performed last year in the Cornell Psychological Laboratory by Dr. Rich. But I hasten to add that the interpretation I place upon my own experiences, as well as upon

the general results of the experiment which Dr. Rich kindly placed at my disposal, is my own, and in no way implies concurrence on the part of Dr. Rich.

In listening to a pure tone I was impressed by the pitch predominance of which Watt speaks, and also by the surrounding aura of volume. I also felt that I could detect differences in *emergence* of the pitch salient from its surrounding volume. This latter effect I designate as *brightness*. These seem to me to constitute three attributes of sound: pitch, volume and brightness. In addition, intensity is clearly attributive, as is also durance. Of these, brightness finds no place in Watt's list, and I must admit that I have no evidence of a correlative uniformity in vibrational frequency, as I have in the case of pitch and volume. How brightness is dependent on frequency and amplitude I do not know. In the main, brightness increases with rise of pitch and decrease of volume, it therefore seems to depend on these two attributes; but intensity may also be involved. Pending further investigation, I am content to accept it provisionally among the attributes because of its descriptive differentiation from the rest. As for Watt's two temporal attributes, one suffices me, because I regard temporal order as a functional effect, partly meaningful, in which kinesthesis probably plays an important rôle.

The attributes of sound here advocated are, then, pitch, volume, intensity, duration, and, tentatively, brightness. A graphic representation will perhaps aid in distinguishing these. (See Fig. 2.) In this minaret-shaped solid the pitch is indicated by the salient peak, while the volumic spread is suggested by the solid form as a whole. The rise of the total mass from its circular base represents intensity. Brightness is the emergence of the salient from the volumic mass. Another figure (see Fig. 3) gives two forms representing like pitch, volume and intensity, but varying brightness. In the first form the salient emerges more clearly than it does in the second. Duration, finally, is mere persistence of the sound in time; dependent upon fluctuations of the stimulus, the physiological response and the attention, providing the basis for rhythms, periods and the like.

Watt also includes quality as an attribute, though he admits but one invariable quality of sound. If a sensation is the sum of its attributes, then quality must be one of them, even at the expense of independent variability. But I prefer to regard the substantial essence of sensation as a fundamental class or modality, rather than as an attribute. I agree with Watt that there are three kinds of sound: tone, noise and vowel, but while he regards these as results of integration, I would refer them to functional interrelationships. Although the attributive nature of the sound determines to a large extent whether it shall be regarded as a tone, noise or vowel, intrinsically it is lacking in any such definition. The act of relationship introduces order. By comparison I determine one pitch to be higher than another, one volume to be greater than another, one tone brighter than another, more intense, or of longer duration. I prefer, then, to speak of three *characteristics* of sound: tonality, vocality and noisiness, rather than of three integrations, or of three attributive qualities.

Tonality is a characteristic of those sounds that can be fused or musically related. It rests primarily upon pitch and volume. As pitch varies from high to low it affords a means of comparison with respect to serial order. But it is not pitch alone that gives rise to musical order. Very high sounds, even when produced by regular periodic vibrations, are notably lacking in tonality. Although differences in height are still evident, volume differences are so vague, owing to their smallness, that they cannot be readily compared. In the lowest range of sound we meet with a similar difficulty, but the reason is not the same. Here volume dominates, and pitch does not emerge sufficiently to define the tone. As all high sounds are too bright, so all low sounds are too dull to lend them the characteristic of tonality. The mind can act in certain ways upon sounds whose pitch and volume are defined. This activity of the mind is musical, and it depends upon the definition of tones. Hence tonality is a characteristic of sounds only when they imply a musical setting. This activity rests chiefly, I believe, on two principles of

correlation. The first is the principle of harmonic interval, and the second that of equal interval, or proportional division.

By *interval* I mean the distinction of two tones whose pitch and volume are both clearly differentiated. This is tonal definition. It is introspectively evident that pitch difference alone does not constitute an interval. Pitch distinction is possible with less than one vibration difference in the stimuli of two tones. As is well known, the liminal increment tends to follow an arithmetical series. One may distinguish with equal ease tones of 250 and 251 vibrations, and tones of 500 and 501 vibrations. Rich has shown[1] that this is not the case with volume. Volume distinctions are not so fine, and they follow a geometrical progression of vibrational frequencies. Thus while the octave of 250 to 500 vibrations would contain more than 250 discriminable differences of pitch, it would contain, from Rich's estimates, but 25 to 50 discriminable volumes. Furthermore, with slight variations of pitch we note no change of tonality. Only when a clear volumic difference is evident does an interval seem to occur.

As regards the origin of the principles upon which tonality may be said to rest, that of harmonic interval obviously suggests the series of partial tones. The common musical setting of the octave is in part due, I think, to the dominance of this interval among the audible partials. The fundamental dominates its overtones. The first interval in the series, as well as the one most frequent among the partials, is the octave. Hence the octave as a special function of relationship may be said to obtain prominence because of the frequency with which the ear must act upon this relation of tones. It may be objected that there are tones in which the even-numbered partials are not conspicuous, but these are rather unusual. A more serious objection is made by Stumpf, who does not admit that frequent association can produce so fundamental an effect. Answering this we have the results of Moore[2] and Valentine[3] to show that adaptation does facili-

[1] Cf. J. of Exper. Psychol., 1916, 1, pp. 13ff.

[2] Psychol. Monog., 1914, 17, no. 73.

[3] *Brit. J. of Psychol.*, 1913, 6, pp. 190ff.

tate functioning. It appears that one can readily learn to change the effect of a not too dissonant into a consonant interval. Still one must rely on something more fundamental than musical practice to explain fusion. I have therefore previously suggested that racial adaptation is also probable.[1] Assuming a certain ease of functioning in the case of the harmonic intervals, it is possible to understand the readiness with which one might be able to detect the differences of timbre that characterize the more significant noises, voices, and clangs of nature. Hence, the most frequent intervals of the partial series may have a certain survival value, because facility in their functioning would thus open a way for recognition of constituents that are not readily fused. V. Hornbostel[2] has sharply criticized my views on the ground that among primitive peoples noises and inharmonic intervals are much more prevalent than tones. But I do not see that this need prevent a gradual evolution in refinement of adjustment and judgment. The facility of harmonic intervals might be of use in perception long before it became a basis for music. Furthermore, we have not one sole principle of tonal order, but two to reckon with. In addition to harmony, there remains the capacity for proportional divisioning of which primitives, especially, make great use.

Granted the octave, however it be founded, the question of smaller intervals is solved in two ways. One is by the harmonic principle in accordance with which other conspicuous intervals of the partial series are the important bases of division: thus, the fifth and the fourth, particularly, lead to the diatonic scale of whole and half tone intervals. The other is the principle of proportional division whereby the octave is divided into a certain number of *equal* intervals, irrespective of harmonics. Hence we find such scales as those of Siam and Java, the first with seven, the second with five equal intervals. Stumpf's investigations[3] leave us with no doubt as to the natural origin of these scales. Even

[1] Psychol. Bull., 1909, 6, pp. 297ff.

[2] *Zsch. f. Psychol.*, 1912, 61, p. 70f.

[3] *Bericht v. d. IV. Kongress f. exper. Psychol.* Leipzig: Barth, 1911, pp. 256ff.

though the divisioning may have been originally influenced by the dominance of the fourth and fifth, as Watt thinks probable,[1] this would only explain the number of steps to be provided in the octave, for these scales have evidently been worked out with a careful regard for equally tempered intervals throughout.

Thus we see that certain Oriental music is more intent upon equal than it is upon harmonic intervals. And this is quite possible, because equal proportions and their multiples are as readily distinguishable as are harmonic intervals. So long as music remains essentially a melodic sequence without musical accompaniment, the harmonic intervals, which alone permit fusion, are not requisite. The melodies of equally tempered scales are atonic, but although they lose the tonic dominance of harmonic organization, they gain freedom in transposibility. It is difficult for us with our very different traditions and usages to appreciate such music, yet I think it quite certain that those who are familiar only with naturally tempered scales find real and logical delight in the employment of intervals that are based upon simple multiples of a common unit.

It seems evident that this principle of proportionality has its origin in the proportional decrease of volumes. Yet it would be incorrect to regard pitch as negligible, for pitch secures to the tone its characteristic salient, and thus contributes expressly to its definition. Volume alone defines the interval in an atonic, no more than it does in a diatonic sequence. While volume difference makes intervals possible, it remains for the mind to choose appropriate functions in determining a specific tonality or musical setting. The main choice is between dominance by the 'harmonic chord of nature,' the partial tone series, or by a simple division of the octave into any small number of equal parts. In either

[1] *Cf.* l. c., pp. 135ff. The interval of three whole tones in the Siamese scale is $514^2/_7$ 'cents,' as compared with 498 'cents' for the true interval of the fourth. Four whole tones in this scale give $685\frac{5}{7}$ 'cents,' as compared with 702 'cents' for the true fifth. The discrepancy in each case is $16\frac{2}{7}$ 'cents.' Comparisons between similar intervals of the Javanese scale show discrepancies in each case of 18 'cents.' The greatest discrepancy between notes of the tempered diatonic scale and those of the scale in just intonation occurs with the sixth, where it is 16 'cents.'

case the octave furnishes the framework, the basic attitude or disposition, upon which these functions must operate. Wherever there is tonality, there is read into the tone an octave setting.

While I have advocated the theory that the octave owes its origin in part to the harmonic series, I am not sure that this is its sole foundation. Although I have criticized Watt's theory of 'balance' as an explanation of all music, I think it not unlikely that the proportional decrease in volume by halves with octave frequencies may be of fundamental importance in lending to this particular interval its outstanding position. Goebel,[1] too, has advanced a theory that bears upon this matter. It is to the effect that in sounding any tone with sufficient intensity, another that is an octave below it is also involved. He believes that portions of the same resonator in the ear act together in producing the two sounds. Hence octaves are subjectively inherent in tones. However this may be, the fact is at least evident that octaves are the most fundamental of intervals, and all effects of tonality relate to them.

While tonality results from the fusion and musical relatedness of intervals, vocality is that characteristic of sounds which defines vowels. As the investigations of Jaensch have shown,[2] vowels are sounds of *regional*, but not of salient pitch. A number of vibrational frequencies varying but slightly from one another unite to occasion the sound we call a vowel. The researches of Köhler,[3] Miller,[4] and others have shown that the chief vowel sounds (continental usage) fall in the order *u*, *o*, *a*, *e*, *i*, each being characterized by a regional pitch beginning around 264 v.d. for *u*, and proceeding upwards by octave steps for each of the others. The significance of these special regions for the outstanding vowel sounds leads Stumpf[5] to suggest the possibility of specially marked *C*-

[1] *Zsch. f. Sinnesphysiol.*, 1911, 45, pp. 109ff.

[2] *Zsch. f. Sinnesphysiol.*, 1913, 47, pp. 219ff.

[3] *Zsch. f. Psychol.*, 1910, 58, pp. 59ff.

[4] D. C. Miller, 'The Science of Musical Sounds.' New York: Macmillan, 1916, pp. 215ff.

[5] *Bericht v. d. VI. Kongress f. exper. Psychol.* Leipzig: Barth, 1914, p. 322f.

Fig. I.

Fig. II

Fig III

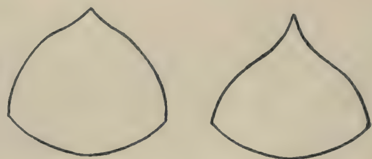

Fig. IV

Fig. V

FIG. 1. Watt's Conception of Fusion.
FIG. 2. Graphic Representation of a Tone.
FIG. 3. Graphic Comparison of two Tones differing in Brightness.
FIG. 4. Graphic Representation of a Vowel.
FIG. 5. Graphic Representation of a Noise.

qualities in the tonal series. Watt,[1] however, remarks that if we but assume some special facility in the articulation of a single regional pitch of this series, the rest would be likely to follow in this order because of the dominating influence of the octave law.

The graphic form of the vowel is indicated in Fig. 4. Pitch is not altogether lacking, but it has no salient of brightness. The sound of the vowel is smooth, for pitch is submerged in the volume. Comparatively, pitch is still distinguishable as an average or regional effect, and, optimally, is that pitch which would be central to this volume if it had a marked salient. The true vowel is not necessarily the only element in a vowel utterance, for Miller has shown[2] that with the higher vowels, *e* and *i*, a pitch predominance of a lo ver order may be present in even greater intensity than is the region of resonance characteristic of the vowel. Yet when the upper region is eliminated, the sound changes to that vowel characterized by the lower region of resonance.

Noisiness, finally, is the characteristic of sounds in which neither a definite salient nor a regional pitch is in evidence. Noises are irregular sound phenomena, and the occasions of such irregularities are numerous. If one increases unduly the range of adjacent frequencies employed to produce a vowel, the sound will gradually take on the character of a noise. If one does not increase the range enough, the sound remains a tone. The ability to bring together adjacent pitches into a definite regional or vowel effect is therefore limited to a certain range of adjacent frequencies. But noise may also occur through the combination of several tones of outstanding pitch. This happens when the combined tones cannot be ordered in accordance with the harmonic principle, *i. e.*, when the constituents do not fuse. Eight notes in the sequence of the major chord fuse into a tonal effect, but eight adjacent notes of an octave, when simultaneously struck, are noisy. In addition we have the noisy effect of incomplete vibrations whose periodicity is confused. Sudden changes

[1] *Brit. J. of Psychol.*, 1914, 7, p. 12f.

[2] *L. c.*, p. 223f.

in intensity, brightness, duration and volume also contribute to such confusion. Indeed, any irregularity that the mind is incapable of mastering by its usual functions of harmonic fusion is an occasion for noise. This includes the noisiness of very high and very low sounds, even though their vibrational frequencies are quite regular. A graphic representation of this condition is offered for comparison with those already given, though it suggests but one among numerous types of noise. (See Fig. 5.)

Although indefiniteness of either pitch or volume is the occasion of noise, Köhler has pointed out[1] that sounds of the high and low regions are not lacking in individuality. For it is here that we find certain of the consonants: *m* sounds are characteristic of the low, and *f*, *s* and *ch* sounds of the high regions. It is also probable that definite blendings of the attributes are responsible for all the other consonants, as well as for a wide range of characteristic and easily recognizable noises.

My conclusion is, then, that within the modality of sound there are to be found the following attributes: pitch, volume, intensity, duration, and probably brightness. With the aid of appropriate mental acts, or functions, the presence of these variables in a sound leads to certain orderly arrangements and usages, resulting in the characterization of sounds as tones, vowels, and noises. While the attributes are revealed from the standpoint of a detached description of structural content, tonality, vocality and noisiness are meaningful implications of reciprocal interactions between mental functions and conscious structures. Lack of functional capacity explains *amusia*, and also some forms of speech deafness. Music, oral speech, and significant noise are all results of comparison and judgment, in which the structural elements of sensory content furnish the basic data.

When the sound possesses a dominant pitch and a definite volume it may be fused and related with other sounds of like nature. It thus acquires the character of tonality. When it possesses regional pitch and regular volumic proportions,

[1] *Zsch. f. Psychol.*, 1913, 64, pp. 92ff.

but no salient, it becomes a vowel. Because the larynx and mouth cavity are capable of producing such regular and characteristic sounds, they have been seized upon as important elements in vocal language. When, finally, the sound is irregular as to pitch and volume, it is described as a noise. But although noises cannot be treated in the octave setting, they are often capable of reduction to characteristic units, recognizable by their individuality as significant vocalizations —the consonants—and likewise as familiar objects of nature.

THE INTENSITY FACTOR IN BINAURAL LOCALIZATION: AN EXTENSION OF WEBER'S LAW[1]

BY G. W. STEWART AND O. HOVDA

University of Iowa

That the ratio of the sound intensities at the two ears is a factor in the localization of the sound source is well known. A number of experimenters[2] have reported investigations in which this ratio was varied and the localization observed, but none of them have attempted quantitative measurements. Previous experiments of the writer[3] on binaural beats indicated that the ratio of intensities when a pure tone is used was not an important[4] factor in binaural localization. This indication was not in harmony with the generally accepted view as to the importance of the intensity ratio, and hence quantitative measurements seemed advisable. This article will be confined to a presentation of these results. The factor of phase difference will not be discussed.

The Method.—The experimental procedure was based upon the belief that a knowledge of the importance of the intensity ratio as a factor in binaural localization should be sought, in the first instance, by the use of simple tones. The results of this paper are for a single frequency, 256 d. v., and the conclusions drawn would furnish only the first step in a complete investigation.

The Apparatus.—The frequency was selected by the circumstance that we already had in our possession a sensitive

[1] From the Physical Laboratory of the University of Iowa. We wish to acknowledge our indebtedness to Mr. Eugene Berry for his assistance as observer.

[2] Tarchanoff, *St. Petersburger med. Wochenschrift*, 1878, No. 43, 353–354. Steinhauser, *Phil. Mag.*, Ser. 5, 1879, 7, 261–274. Urbantschitsch, *Pflüger's Archiv*, 1881, 24, p. 579. Kessel, *Arch. f. ohrenheilk.*, XVIII., 1882, p. 120. Matsumoto, Studies from the Yale Psychological Lab., 1897, No. 5. Stenger, *Zeitsch. f. Ohrenheilk.* XLVIII., p. 219. Rayleigh, *Phil. Mag.*, 1907, 13, p. 217. Ferree and Collins, *Am. J. of Psychology*, 1911, 22, p. 250. Numerous other writers refer to the subject.

[3] Stewart, *Phys. Rev.*, 1917, 9, 502–528.

[4] Rayleigh, *loc. cit.*, shows that it cannot be an important factor for 128 d. v.

Rayleigh disc[1] made and adjusted for 256 d. v. per second. The source of sound was a tuning fork of this frequency. Two sounds, one for each ear, were conducted from the fork by glass tubes, rubber tubing and binaurals, which, in a portion of the experiments, were inserted in the ears.

In order to obtain as pure a tone as possible and yet to maintain the vibration of the fork by electrical means, the common device of two forks in tandem, as shown in Fig. 1,

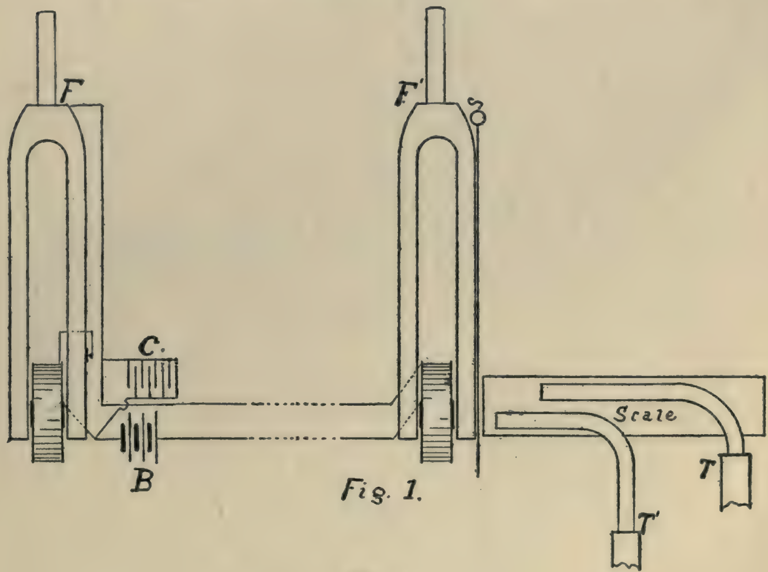

FIG. 1

was used. The auxiliary fork, *F*, carried the electrical contact and the circuit was completed by the battery and two magnet coils, one for each fork. Two glass tubes were placed in the plane of vibration at the side of one prong of the fork, *F′*, used as the sound source. One of the tubes was stationary and the distance of the other from the fork could be altered. Thus the flow of energy into one tube was modified at will and the relative intensities made any selected value within the range of the apparatus. The screen, *S*, could be inserted

[1] This disc is described in an article by Stewart and Stiles, *Phys. Rev.*, N. S., 1913, 1, p. 311.

and removed from the space between the fork and the end of the glass tubes. The terminus of the glass tube, T', was permanently fixed at a distance of 1.02 cm. from the fork and the position of the terminus of the other glass tube was varied from 0.20 cm. to 2.20 cm. from the fork. The sound was led from the glass tubes to the binaurals through approximately 3 meters of rubber tubing in each branch. The relative intensities or fluxes of energy in the binaurals were tested by the Rayleigh disc. Into the open end of the disc was inserted a stationary glass tube which will be called the 'disc tube.' Comparative measurements of the flow of energy through either binaural could be tested by connecting the binaural to the disc tube by a short rubber tube. The deflections of the disc were small enough to be considered as proportional to the intensities.

Binaurals constructed in order to avoid contact in the external meatus are shown in Fig. 2. B is a piece of thin

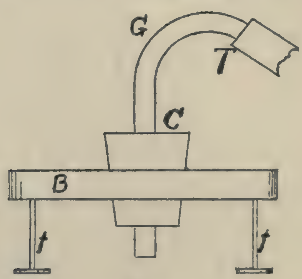

FIG. 2

triangular board, carrying at the corners three feet, f, which rest upon the skull. The sound enters through the rubber tubing, T, and passes through the glass tube, G, the end of which can, by adjustment in the cork, C, be placed near the opening of the ear. The real advantage of the binaurals is not that they completely isolate the skull, but that they present the sound to the ears without closing the external meatus. Nevertheless, the sound conveyed by the feet to the skull is vanishingly small and without influence.

In a part of the experiments the binaural observer was in the same room with the forks, but when the non-contact

binaurals were used the observer was in an adjacent room, in which the sound from the forks was sub-liminal. In front of the binaural observer was placed a chart marked with radial lines representing angular displacements from the median plane. The second experimenter was stationed at the sound source, and had the duty of varying the intensity of sound received at one ear by altering the distance of the adjustable tube at the fork. By resistance adjustment he also kept the amplitude of the fork approximately constant, this being a precaution to avoid any large variations in the flux of energy from the fork.

In each set of experiments fifty different values of intensity-ratio were used. In order that these values might be properly distributed in a given set the desirable displacements of the tube were chosen and these arranged 'at random.' The binaural observer was thus wholly without any information that would influence him in favor of a certain angular displacement.

Several assumptions and approximations were made in the arrangement of apparatus and the interpretation of results. They are as follows:

1. It was assumed that, for a given adjustment, the intensity-ratio of the stimuli at the two ears is the same as the ratio of intensities produced at the Rayleigh disc. The assumption seems to be entirely warranted.

2. It was assumed that the vibrations produced at the ears were alike in phase. This is only approximately true. The adjustable tube at the fork was displaced not to exceed one centimeter from its mean position. Thus the maximum change in phase produced by the altered length of path was less than 3°. Since former experiments with change of phase[1] indicates that the angular displacement in localization by a difference of phase is of the same order as the phase difference itself, one can be confident that the error of 3° does not modify the conclusions of our experiments.

3. In the interpretations of the results it is assumed that our method of producing intensities at the ear is physically

[1] G. W. Stewart, *Phys. Rev.*, 1917, 9, pp. 502–508.

equivalent to that of moving a source of sound about the head, with the important difference, however, that in the method used the phase difference is negligible. This assumption disregards any effect produced by the impinging of waves upon the skull, which would occur in the case without binaurals. But experiments with the external meatus carefully

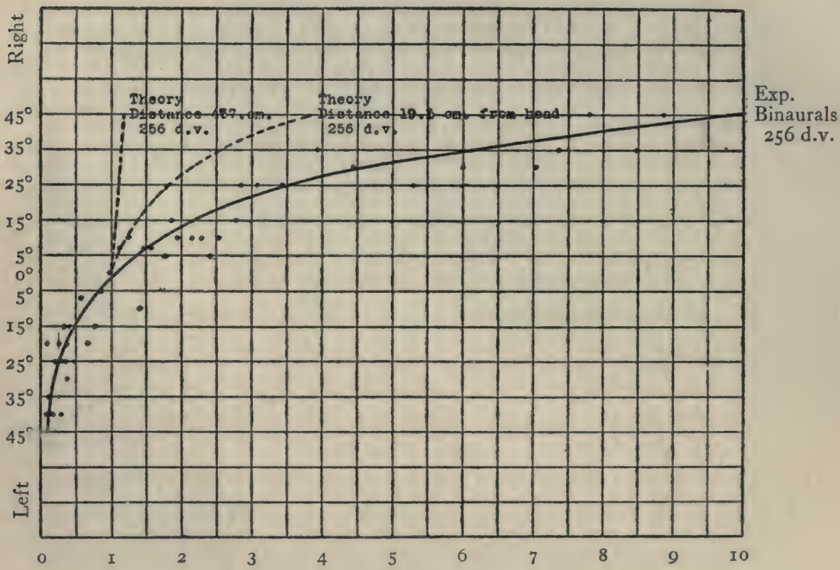

FIG. 3. Relative intensities.

stopped proved that the intensity of sound conveyed from the skull to the drum-skin under such circumstances was subliminal.

The Results.—The results first to be mentioned are those made with ordinary stethescope binaurals. The screen at the fork was opened for one or two seconds only, the operation being repeated with silences of perhaps one second each until the binaural observer indicated in some manner that he had arrived at an opinion concerning the apparent angular displacement from the median plane. In Fig. 3 the observations of one set are shown by dots and the mean results by the full-line curve.

The ordinates represent the apparent angular displacements from the median plane of the source of the blended sound and the abscissæ the ratio of intensities of the stimuli at the ears. The values last named we obtained from a knowledge of the distances of the movable tube as shown in the scale, Fig. 1, and a calibration curve taken with the aid of the Rayleigh disc. This curve was obtained by repeated measurements of the relative intensities with various distances on the scale, Fig. 1. The calibration measurements and curve are omitted, inasmuch as they will not be discussed in any manner. The error in the knowledge of any actual intensity-ratio does not exceed five per cent. and is usually much less.

The most striking result given by Fig. 3 is the relatively large change in intensity necessary to produce displacement in localization. The intensity at one ear had to be ten times that at the other in order to produce a displacement of 45°. This is surprising, for an intensity-ratio as great as 10 to 1 at the ears never occurs in our common experiences. By way of comparison, let us ascertain what values of intensity-ratio would be actually obtained by displacing a source of sound 45° from the median plane. For such values we are dependent upon a theoretical investigation,[1] in which were found the relative values of intensities at various points of a rigid sphere 60 cm. in circumference, with the source at several distances, r. In order to utilize the results of the theoretical investigation, we may select two points diametrically opposite on the sphere and assume the source to be carried about the sphere in a plane containing this diameter. From Fig. 2 of the reference just cited, it is possible to get the values of the intensities at these diametrically opposite points for a source of a wave-length 120 cm. (or, nearly enough), the frequency here used at distances of 477 cm. and 19.1 cm. from the sphere. It is a simple matter then to take the ratio between these intensities for any angular displacement of the rigid sphere corresponding to the apparent angular displacement observed and shown in Fig. 3. The dotted line curves in

[1] Stewart, *Phys. Rev.*, 33, p. 473.

Fig. 3 show these computed values of intensity-ratio and thus can be assumed to represent approximately the ratios of intensities at the ears with a source of sound 256 d. v., at distances of 477 cm. and 19.1 cm. It is apparent that with a displacement of 45°, the change in the theoretical ratio of intensities is 20 per cent. and 300 per cent. for the distances

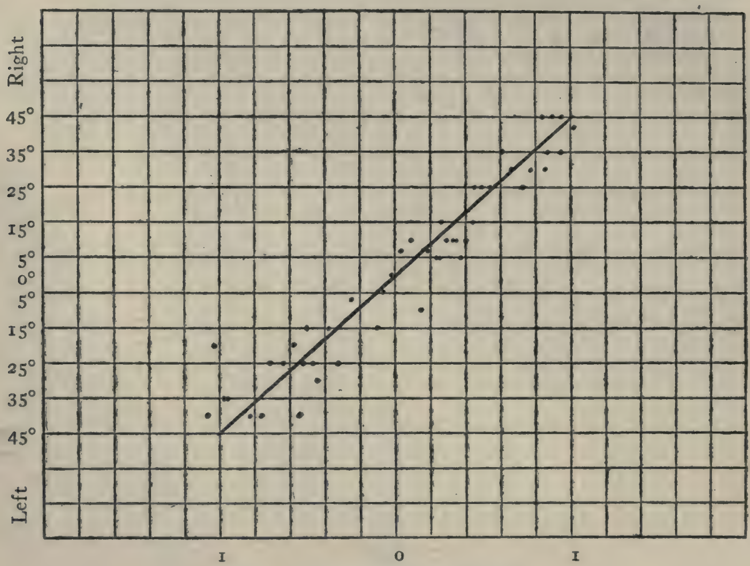

FIG. 4. Log of relative intensities.

from the center of the head of 477 cm. and 19.1 cm. respectively. But in our present experiment the change in the ratio required to produce apparently this same displacement is 1,000 per cent. It is perhaps fair to compare our present experimental intensity-ratio with the theoretical values obtained when the source of the same frequency is placed at the not unusual distance of 477 cm. We have then to compare an experimental change of intensity-ratio of 1,000 per cent. with the theoretical of 20 per cent. The comparison shows that the localization of a simple tone of 256 d. v. cannot be due to the intensity-ratio, for a change in ratio of 20 per cent. as shown by these experiments, would produce an ap-

parent displacement of less than 4°. But our experiments deal with intensity changes *only*. Hence our conclusion is that the intensity factor alone is of relatively small importance with a simple sound source. The work 'relatively' is used advisedly for it is well known that the difference of phase, when that alone occurs, is a very important factor.[1]

If the experimental results presented in Fig. 3 are plotted with the logarithms of relative intensities as abscissæ, the results are as shown in the accompanying Fig. 4.

Clearly the mean is best represented by a straight line. This leads at once to the interesting conclusion that the angular displacement, θ, can be expressed in terms of the right and left intensities, I_r and I_e respectively, in the following equation.

$$\theta = \mathrm{K} \log \frac{\mathrm{I}_r}{\mathrm{I}_e}, \qquad (1)$$

where K is a constant.

Before discussing the significance of this law, a record should be made of all the observations in our experiments. There were three observers, and each took a set of observations similar to that shown in Fig. 4. Also each took a set of observations with the non-contact binaurals as shown in Fig. 2. All the observations amply verified (1), but the slope of the non-contact-binaural-curve was generally slightly different, with one observer less and with another greater. Curves indicating the results obtained by one observer are shown in Fig. 5.

Here the slopes of the three full-line curves are 0.81, 0.91 and 0.75, averaging 0.82. The slope of the dotted line, or the non-contact binaural, is 0.63. The results with the non-contact binaurals show an even greater insensitiveness to changes of intensity at the ears and hence the contact binaurals do not lead to incorrect conclusions because of the closing of the external meatus. From the dotted line in Fig. 5, we find the ratio of intensities required to cause a displacement of 45° is approximately 25 to 1.

The results obtained with the other two observers were

[1] See references given by Stewart, *Phys. Rev.*, Second Series, 1917, 9, p. 502.

similar. The average slopes of the contact binaural curves were 0.96 for one and 0.93 for the other observer.

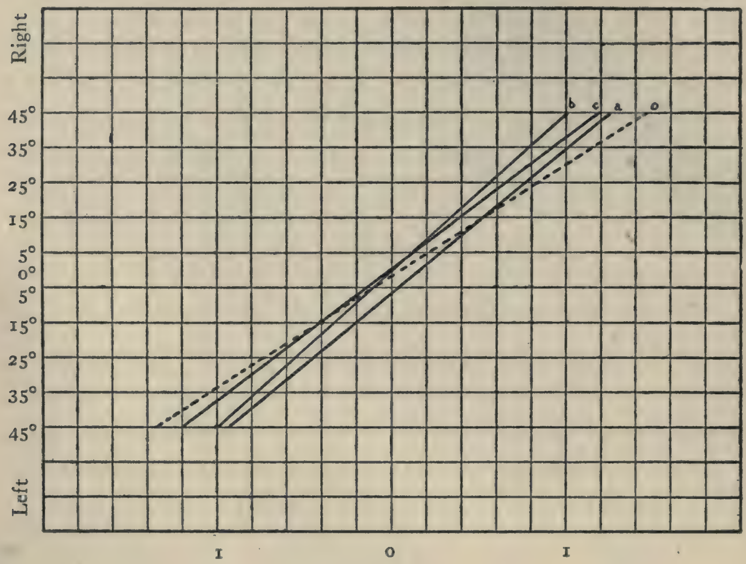

FIG. 5. Log of relative intensities.

In the experiments evidence was found that under certain circumstances the results might be best represented by two straight lines meeting at approximately 0° on the chart. These unusual results will not be discussed in this paper for it is believed that the results already shown represent the normal case.

Equation (1) reminds the reader of Weber's law. In its integrated form it states that the response is proportional to the logarithm of the stimulus. Its apparent truth is found in experiments on vision, audition, pressure, smell, and kinæsthetic complexes, indicates that the law is one of the nervous system and is not dependent upon the nature of the peripheral organs. The discovery expressed in (1) is, however, an interesting and perhaps an important extension of Weber's law, for here we find that the response of the individual as indicated by the constructive angular displacement

is proportional to the logarithm not of one stimulus, but of the ratio of the two stimuli. The above results open up many questions of interest both to the physicist and to the psychologist. The experimental work must be extended until the importance of intensity and phase relations, both singly and together, both with simple and complex tones and for all frequencies is clearly understood.

THE MENTAL INDICES OF SIBLINGS

BY RUDOLF PINTNER
Ohio State University

In the course of giving group tests to a large number of children the writer had the opportunity to compare the performances of many groups of brothers and sisters. The six tests used were (1) Rote Memory Test for concrete words; (2) Digit-Symbol Test; (3) Symbol-Digit Test; (4) Word Building; (5) Easy Opposites; (6) Cancellation. The results obtained with this group of tests, the percentiles for each age and the method of calculating the mental index of each child have been described elsewhere[1] and need not be repeated here.

Out of a great number of grade-school children tested 180 families, of which two children had been tested, and 37 families of three children were found. There were only about five families of four children and these have not been considered. The families of which two or three children have been tested will be dealt with here. A mental index of 50 denotes average ability for any age. Indices above or below 50 indicate abilities above or below the median ability. Is there a greater similarity between the indices of siblings than those of unrelated children?

The average deviation of the mental indices of a family may be taken as one measure of the resemblance of mental ability. The average deviations for the families of two and three were calculated. These average deviations are compared with the deviations obtained by choosing any individuals at random. In the chance selections the cards of unrelated children were thoroughly mixed and then taken in order. The chance selections compared with the families of three contained, of course, three individuals, and similarly

[1] Pintner, R., 'The Mental Survey.' D. Appleton and Co. 1918.

those compared with the families of two contained two individuals. The children attended two different schools and the schools were treated separately and then combined. The averages of the A.D.'s for the various groups are:

	Families of Three		Chance Selections	
	No. of Cases	Average A.D.	Average A.D.	No. of Cases
School *A*	20	11.1	13.8	20
School *B*	17	12.6	13.4	17
Both schools	37	11.7	16.7	37
			14.9	37
			13.6	37

	Families of Two		Chance Selections	
	No. of Cases	Average A.D.	Average A.D.	No. of Cases
School *A*	91	9.8	11.9	91
School *B*	89	9.9	11.6	89
Both schools	180	9.8	12.0	180
			12.3	180
			11.7	180

In School *A* there were twenty families of three and the average of the average deviations of the twenty groups is 11.1. A chance selection of twenty groups of three unrelated individuals each in the same school gives an average A.D. of 13.8. For the total number of families of three the average A.D. is 11.7 and this is compared with three different chance selections each of 37 groups of three unrelated children. In the same way the 180 families of two are compared with three chance selections each of 180 groups of two unrelated children.

It will be seen that the A.D.'s of the siblings are smaller than those of unrelated children. The difference is, to be sure, not very great, but it is present in each school and both in regard to the families of two as well as to the families of three. The average A.D.'s of the chance selections are remarkably similar to each other, and this fact, taken in conjunction with the number of chance selections calculated, seems to show pretty conclusively that the greater resemblance of the mental indices of siblings is not due to chance. At the same time this resemblance is not very marked.

There are many families in which the mental indices of the children differ widely. The largest A.D. for any pair calculated was for a pair of siblings, one of which was feeble-minded and the other slightly above average.

In another treatment of the same data, Yule's[1] association coefficient,

$$Q = \frac{(AB)\ (\alpha\beta) - (A\beta)\ (\alpha B)}{(AB)\ (\alpha\beta) - (A\beta)\ (\alpha B)},$$

using above and below an index of 50 as the dividing line, was used. This coefficient has been calculated for the 180 families of two and for various chance selections with the following results:

	Q	n
School A	.47	91
School B	.28	89
Both schools	.39	180
Chance selection of sibs	.09	180
Chance selection of sibs	.14	222
Chance selection of unrelated children	.14	151
Chance selection of unrelated children	.19	300

The chance selection of siblings means that the mental indices of the siblings were used, but no two siblings were ever paired. The chance selection of unrelated children excludes the mental indices of the siblings. Here again we note that the coefficient for the siblings is considerably higher than that obtained by any of the chance selections.

The above facts may also be expressed in this manner. There were 114 pairs of siblings, or 63 per cent., in which both members of a pair had a mental index above 50 or both below 50; and there were 66 pairs, or 37 per cent., in which one member had an index above and the other an index below 50. In a chance selection of 180 unrelated pairs (using the indices of the siblings) 99 pairs, or 55 per cent., had both members either above or both below 50; and there were 81 pairs, or 45 per cent., in which one member was above and one below 50.

The Pearson coefficient for the 180 pairs of siblings is $r = .22$, with a P.E. of .02. This correlation is slightly

[1] Yule, G. Udny, 'An Introduction to the Theory of Statistics,' 1916, p. 38.

lower than those obtained by Thorndike for three single tests, namely 29, 30 and 32. Earle's coefficients for spelling ability ranged from .22 to .50. It is much smaller than that obtained by Peters for memory, namely $r = .36$. In the same way the association coefficient of $Q = .39$ for our siblings is smaller than the coefficient of $Q = .53$ obtained by Peters for school grades.

On the whole the general intelligence of siblings is more nearly alike than the general intelligence of unrelated children selected by chance. This greater resemblance is unquestionably due to inheritance, for we are not here dealing with activities that are much influenced by school training. A larger number of tests and a better technique than the group-test method would undoubtedly have shown a greater resemblance, but the resemblance of the mentality of siblings is clearly present, and we have found it even by means of our rough survey tests.

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The Psychological Review

ASSOCIATIVE AIDS: II. THEIR RELATION TO PRACTICE AND THE TRANSFER OF TRAINING

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III

The methods and materials for this experiment were the same as those described in my paper on 'The Relation of Associative Aids to Learning, Retention, and other Associations.' As stated there, the tests were repeated every day for six days, and on the sixth day, the *B*, *D*, and *U* tests were given in addition. The former enable us to detect the effect of repeated learning or practice upon associative aids, and the latter enable us to detect their influence upon the transfer of training in so far as this means the effect of learning things in one connection upon learning them in a different connection. The solution of these two problems will form the burden of this paper.

The nature of our experiment enables us to measure practice effect in various ways and also to isolate several factors influencing the cause of practice. The former may be measured (1) by the average number of repetitions, *R*, per pair for each day; (2) by the average reaction time, *T*, per pair for each day, and (3) by the number of pairs correctly remembered each day, *NC*. Since the time was taken for every reaction, wrong or right, we were able to tell to what extent practice effect is due to the elimination of errors and to what extent it is due to the improvement in the times of the correct reactions, *CR*. All of these measures were cal-

culated for each individual for each day and test, and their averages for all of the subjects of the experiment. The reliability of these averages was measured by a calculation of the A.D.'S for the group averages. In addition, the A.D. of the Ave. L.T. or T. of each individual for each day was calculated and this measure is called *TAD*. The group average of *TAD* and its A.D. were also calculated. The *TAD* therefore measures the variation of the individual in his own L.T.'s, and the A.D. of the group average of the *TAD* measures the variation of the individual *TAD*'s among each other. The group averages for each day and test of all these measures together with their A.D.'s appear in Table VIII. Following our custom, we have kept the measures of each series separate and in the last section of the table we have given the general averages for all the series.

If we imagine curves for the respective averages in Table VIII, it will be seen that the *R*, *T*, *TAD*, and *CR* curves all

TABLE VIII

SB

	1	2	3	4	5	6	*B*	*D*	*U*
R......	1.89	.51	.16	.08	.03	.05	.14	.98	1.26
AD....	.58	.21	.12	.10	.04	.08	.09	.62	.66
T......	7.25	4.25	2.55	1.99	1.65	1.65	2.47	7.85	8.43
AD....	3.08	1.26	.54	.64	.69	.49	.62	4.61	2.30
TAD...	4.30	2.02	1.10	.92	.86	.80	1.68	4.55	5.69
AD....	2.04	.95	.70	.60	.68	.67	1.07	2.05	1.16
CR.....	2.35	2.13	1.73	1.59	1.53	1.46	1.89	3.75	3.38
AD....	.62	.92	.46	.34	.42	.38	.39	1.05	.44
NC.....	4.56	5.92	8.44	9.16	9.65	9.55	8.33	3.55	2.70
AD....	1.44	1.08	1.44	.80	.56	.76	.55	1.70	.62
N......	250	250	250	250	200	210	130	130	70

RC

	1	2	3	4	5	6	*B*	*D*	*U*
R......	1.67	.50	.15	.06	.04	0	.26	1.08	1.00
AD....	.29	.35	.19	.09	.05	0	.12	.84	.24
T......	5.34	3.87	2.74	2.21	1.91	1.54	3.90	7.38	10.60
AD....	2.90	.76	1.46	.85	.39	.28	1.59	2.77	3.14
TAD...	3.19	2.15	1.45	1.37	.79	.50	2.42	5.08	5.64
AD....	1.85	1.29	1.15	.90	.47	.30	1.97	1.87	3.31
CR.....	2.05	2.22	2.01	1.85	1.75	1.51	2.36	2.55	2.64
AD....	.51	.46	.44	.65	.29	.24	.75	.81	.89
NC.....	5.09	6.59	8.77	9.36	9.47	9.84	7.77	4.16	2.78
AD....	.62	.65	1.44	1.07	.65	.28	.75	2.66	1.18
N......	220	220	220	220	180	180	90	70	70

WS

R......	2.55	.76	.37	.23	.18	.13	.51		.77
AD....	.59	1.02	.25	.15	.15	.13	.52		.55
T......	11.89	6.25	3.90	3.10	2.49	2.31	3.52		8.32
AD....	3.85	3.76	2.44	1.52	1.20	1.03	4.46		4.11
TAD...	6.98	3.94	2.77	2.16	1.54	1.33	3.24		4.81
AD....	2.62	2.57	1.93	1.72	1.15	.97	2.04		2.87
CR.....	2.82	2.01	1.96	1.77	1.66	1.60	1.96		3.65
AD.....	.73	.40	.53	.77	.42	.21	.51		1.27
NC.....	2.52	6.63	7.11	8.15	8.76	8.63	7.00		4.86
AD....	1.65	1.86	1.57	1.48	1.01	.73	1.76		2.52
N......	270	270	270	270	240	230	240		220

WP

R......	4.30	1.40	.43	.23	.20	.13		1.16	
AD....	1.08	.75	.29	.24	.20	.15		.88	
T......	17.50	8.29	4.20	3.15	2.66	2.09		13.36	
AD....	7.25	4.80	1.99	1.41	.94	.71		5.96	
TAD...	7.78	4.45	2.41	1.83	1.36	1.07		7.05	
AD....	3.81	2.32	1.42	1.08	.76	.42		3.80	
CR.....	3.24	2.53	2.41	2.11	1.94	1.75		3.68	
AD.....	1.13	.75	.82	.63	.45	.57		1.38	
NC.....	1.07	3.30	7.00	8.11	8.61	9.00		2.50	
AD....	.76	1.90	2.07	1.51	.92	1.04		.93	
N......	270	270	270	270	250	220		240	

General Averages

	1	2	3	4	5	6	*B*	*D*	*U*
R......	2.60	.79	.28	.15	.11	.08	.23	1.41	1.01
AD....	.58	.58	.21	.15	.11	.09	.24	.78	.48
T......	10.49	5.67	3.35	2.64	2.18	1.90	3.29	9.53	9.12
AD....	4.29	2.52	1.61	1.11	.86	.63	2.22	4.45	3.08
TAD...	5.56	3.14	1.93	1.57	1.14	.92	2.45	5.56	5.38
AD....	2.58	1.78	1.30	1.07	.77	.59	1.69	2.57	2.45
CR.....	2.62	2.22	2.03	1.79	1.72	1.58	2.06	3.33	3.22
AD.....	.75	.63	.56	.49	.39	.35	.55	1.08	.87
NC.....	3.31	5.61	7.83	8.69	9.12	9.26	7.70	3.40	3.45
AD....	1.13	1.37	1.64	.97	.78	.70	1.02	1.76	1.44
N......	252	252	252	252	217	211	153	146	120

R = No. of repetitions per pair
T = Learning time in seconds
TAD = Ave. of *AD*'s of individuals in their *T*'s per pair
CR = Reaction time for correct responses
NC = No. of pairs correctly remembered
N = No. of pairs upon which averages are based.

have the same general shape. They fall rapidly for the first three days and very slowly for the other days. In the sixth day, they turn their direction, rising gradually for the *B*, *D*, and *U* tests respectively. The *NC* curve has just the reverse shape. These characteristics are shown clearly in Chart I,

which is based on the general averages at the bottom of table VIII. If we measure the difficulty of the different series by their height from the zero abscissa line, the order of difficulty beginning with the easiest is *SB*, *RC*, *WS*, and *WP*. It is noteworthy that the *CR* curve is the lowest one and has the flattest shape. But there is a very gradual fall from day

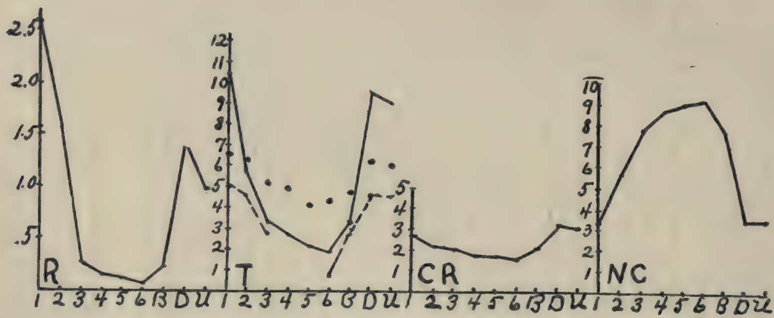


CHART I. Based on general averages in Table VIII. *R* = No. of repetitions per pair. *T* = Learning time in seconds. *CR* = Reaction time for correct responses in seconds. *NC* = No. of pairs correctly remembered. – – – – Dashes = No. of associative aids, general averages, Table IX. . . . . Dots = No. of associative aids, general averages, Table X. Abscissas = Days of practice and *B*, *D* and *U* tests.

to day. This shows that improvement is due principally to the diminution of error but it is also due in part to a shortening of the correct reaction from day to day, *i. e.*, a mechanizing of the correct responses.

If we wish to discover the relation of associative aids to the practice effect just described, we can do so by calculating the average frequency of the pairs that were associatively learned from day to day. As stated above, we recorded, after the learning of each series, the associative aids on the first, second, third, and sixth days for eighteen subjects, and after each response on each day for 9 subjects. The first procedure prevented the creation of artificial reports and interference with the natural course of associative aids by a continual stimulation of them. On the other hand, the second procedure enables us to picture the details of the course of associative aids that would be forgotten and omitted in the

former procedure. The results of the first procedure are given in Table IX and those of the second in Table X.

TABLE IX

TABLE SHOWING AVE. NO. OF ASSOCIATIVE AIDS IN THE 1ST, 2D, 3D, AND 6TH DAYS, AND IN THE *B*, *D*, AND *U* TESTS. FIRST PROCEDURE

SB							*RC*						
1	2	3	6	*B*	*D*	*U*	1	2	3	6	*B*	*D*	*U*
5.68	4.88	4.00	0.84	1.93	4.30	4.25	6.92	6.08	3.52	0.70	4.40	5.71	4.55
$N =$	250			140	63	108	$N =$	250			50	140	63
WS							*WP*						
4.60	4.32	1.66	0.72	2.40		5.00	3.68	3.33	2.44	0.56		4.10	
$N =$	250			150		171	$N =$	250				171	

From Table IX it will be seen that the frequency of associative aids decrease very little on the first two days, but on the third day they take a sharp drop, and by the sixth day have almost completely slumbered away. However, they are again slightly awakened by the *B* test and have become fully active in the *D* and *U* tests. There is thus a very close parallelism between the *R* and *T* curves, on the one hand, and the curves for the frequencies of associative aids on the other. This is shown in the dots and dashes with the *T* curve in Chart I. They are based on general averages calculated from Tables IX and X. This relationship means that associative aids disappear with practice and that their disappearance shortens the reaction time. My records

TABLE X

TABLE SHOWING AVE. NO. OF ASSOCIATIVE AIDS EACH DAY FOR INDIVIDUALS FROM WHOM VERBAL REPORTS WERE TAKEN AFTER EACH RESPONSE. SECOND PROCEDURE

SB									*RC*								
1	2	3	4	5	6	*B*	*D*	*U*	1	2	3	4	5	6	*B*	*D*	*U*
8.00	7.20	5.83	5.20	6.00	6.30	6.30			9.00	7.50	5.50	5.50	2.50	2.00		6.50	
$N =$	60			40		40			$N =$	20						18	
WS									*WP*								
5.00	4.40	4.25	3.75	2.71	3.66	3.20		6.33	4.86	6.37	5.75	5.50	5.50	5.66		6.33	
$N =$	80			60	60	54			$N =$	80			60	60		54	

show that a pair that was accompanied by an associative aid on the sixth day always had a longer reaction time than one that was not so accompanied. This is what we should expect, for, other things being equal, the more middle terms between a stimulus and a response, the longer the reaction time would reasonably be. From this principle as well as from the facts of Table IX., it is safe to assume that the disappearance of these middle terms is another factor conditioning the rate of improvement from day to day.

That the presence of associative aids should shorten the L.T. on the first and second days and lengthen it on the sixth day is not at all a contradiction in terms if we remember their natural course and the nature of the learning process. But in any case, however contradicting this may appear from the standpoint of logic, the facts of the matter force us to accept its rationality. During the early stages of learning, associative aids supply a connecting link between two unrelated terms and thus make an easy transition from one term to the next. If there is no such connecting link, the learner usually has to wait a little while until the response comes, and besides, has to make an effort to recall a fact, which always consumes a long time in comparison to a case where the connections are ready at hand. In neurological terms, we may think of an associative aid as supplying a roundabout conduction unit of low resistance between two newly stimulated cells. When one of these is stimulated the excitation travels readily to the other. When there is no associative aid, the excitation of the neurone for the one term is blocked and must travel slowly to the neurone for the next term because of the high resistance, and therefore results in a long reaction time. After a period of exercise, however, the synapses of the neurones for the new terms become open and the excitation travels rapidly and directly from the one to the other, producing the shortest possible reaction. A roundabout path is no longer necessary, and if the excitation travels through it, it requires that much more time. The associative aid is thus a means of opening the synapses or for reducing the resistance of the newly stimulated neurones, and when it has

once performed that function, the connection becomes direct. Or on the psychological side, the function of associative aids is to connect new terms, and when they have made that connection, they disappear.

From Table X the decrease in frequency of the associative aids is much slower than in Table IX. There is, however, a decrease, and it is the most marked in the *RC* or easiest series. This indicates that with this procedure it would simply take a longer time for the associative aids to disappear. The slow decrease shown in this table in comparison with Table IX makes it evident that the ever-recurring demand after each response to report what was thought of between the stimulus and the response not only kept the associative aids in a high degree of excitation but also engendered an attitude or set that favored such an excitation.

Having sketched the general course of associative aids as it is influenced by repeated learning, we may now illustrate it by examples from our records.[1]

First Procedure

	March 10	March 11	March 12	March 16
	5. Clean 1″			
	4. No 2″	4. Clean 1″		
	3. No 8″	3. No 3″		
	2. No 2″	2. No 4″		
Mistake clean	1. 0″	1. No 4″	1. Clean 1″	1. Clean 2.5″
First day	Nothing			
Second day	"			
Third day	"			
Sixth day	"			
	2. Tarry 2.5″			
Simmer tarry	1.	1. Tarry 2″	1. Tarry 1″	1. Tarry 1.5″

First day: Thought of something on the stove and tarrying a long time.
Second day: Same.
Third day: Same.
Sixth day: Nothing.

	March 10	March 11	March 12	March 16
Miss Jen. 5 P.M.	3. Time 4.5″			
	2. No 4.5″			
Galley time	1.	1. Time 3″	1. Time 2″	1. Time 1″

First day: Thought of boat with galley slaves, and it took a long time to get in the boat.

[1] For an explanation of the arrangement, see this volume, p. 139.

Second day: Same.
Third day: Same.
Sixth day: Nothing.

	3. Betide 1″			
	2. No 5″	2. Betide 1″	2. Betide 1″	
Space betide	1. 0″	1. No 8″	1. No 8″	1. Betide 1″

First day: Space between the tides.
Second day: Same after prompting.
Third day: Nothing.
Sixth day: Nothing.

Second Procedure

Mr. Teh.

8–9 A.M.	May 18	May 19	May 20
	2. Coffee .8″		
Radish coffee	1. 0″	1. Coffee 2.6″	1. Coffee 1.6″
	May 21	May 22	May 24
	1. Coffee 1.7″	1. Coffee 1″	1. Coffee .5″

First day: Saw a radish and a cup and a saucer on the table.
Second day: Just the word coffee came into my mind.
Third day: Saw a radish, coffee came immediately.
Fourth day: Saw a red radish and a table, then the word coffee came.
Fifth day: Nothing.
Sixth day: Nothing; expected it.

	May 18	May 19	May 20	May 21
	2. Wall 2.3″			
Soup wall	1. 0.0″	1. Wall .6″	1. Wall 1″	1. Wall 1.2″
	May 22	May 24		
	1. Wall 1.6″	1. Wall 1.8″		

First day: I immediately connected these with serve-hold, waiter holding soup and spilling it on wall.
Second day: Just tray and then wall came.
Third day: Same.
Fourth day: Vague image of tray, then wall came.
Fifth day: Nothing.
Sixth day: Thought of Zool. Lab., then Miss Wall. She makes so much noise over there.

	May 18	May 19	May 20	May 21
	5. Lek 3.4″			
	4. Rem 13.6″			
	3. Rem 2.4″	3. Lek 2.3″		
	2. Zum 3″	2. No 10.6″		2. Lek .8″
Yab lek	1. 0.0″	1. No 4.6″	1. Lek 3.4″	1. No 8.″
	May 22	May 24		
	1. Lek 1.4″	1. Lek 2.3″		

First day: 2. Zum seemed right.
3. Rem seemed right.
4. Response has an *e*, had said rem but it was wrong, but I could think of nothing else.
5. Thought of rem, right response begins with letter before *m*, so I got it. Thought of this after last prompting.

Second day: 1. Only yab.
2. Thought of rem, not right because I tried it twice before; don't know.
3. Thought of rem, not right, response has an *e* in it and begins with first consonant before *m*.

Third day: Same.
Fourth day: Same, but could not think of lek.
Fifth day: First rem, then lek immediately.
Sixth day: Rem, then *lek*.

Miss Bro.

3–4 P.M.	May 24	May 25	May 26	May 27
	2. Stencil 2.4″	Design		
Kimono-stencil	1. 0.0″	1. Stencil 2.6″	1. Stencil 1.6″	1. Stencil 1.4″
	May 29	June 1		
	1. Stencil 1.8″	1. Stencil 1.0″		

First day: Had picture of a design stenciled on kimono.
Second day: Same.
Third day: Thought of design, then stencil.
Fourth day: Only stencil.
Fifth day: " "
Sixth day: " "

The above examples enable us to picture the course of associative aids as they are influenced by repeated learning. The first procedure shows us that in some cases they come up almost in the same way for three experiment days, but have wholly disappeared by the sixth experiment day. In some cases they occur only on the first and second days; and in others only on the first day. This is in agreement with the frequency curves for the four series of pairs learned by 18 subjects. There is a gradual fall in the curves for the first three days, but by the sixth it touches the zero line. The second procedure has the advantage in enabling us to tell not only what happens to the course of associative aids on the fourth and fifth experiment days but also how they are influenced by repeated promptings on the same day. Mr. Teh.'s reports are enlightening in this connection. This

student had the benefit of two semester's work in both theoretical and laboratory psychology and had learned to become a skillful observer. For example, the pair 'troll-blast,' learned by him required four promptings, *R*'s, on the first day and an L.T. of 26.7″. Before the second *R*, he could think only of the stimulus, 'troll.' Before the third *R*, he could remember only that he was prompted. Before the fourth *R*, he again could think only of the stimulus. But he now succeeds in devising an association. He thinks of trolls as mountain spirits in one of Ibsen's dramas. The idea of mountain calls up mining, and mining calls up the idea of blast. He now has a connection between troll and blast, and the next time he responds correctly. On the second day, he recalls this association at once and has an L.T. of 2″. By the third day it has almost slipped away, but it comes back slowly and he has an L.T. of 5″. On the third day, he thinks of placing 'trolls' in a book, which immediately calls up blast. The intermediate links of 'trolls' in the mountains and mining, and of mining and of blast, have disappeared. On the fifth day, he thinks only of 'trolls' in a book. The idea of placing them definitely has disappeared. On the sixth day the response is immediate without any association, the L.T. being only 1″. The learning of 'yab lek' is another example showing in detail a similar psychological process. A third example may be taken from the learning of 'soup wall.' On the first day, Mr. Teh. thinks immediately of a previous pair, 'serve-hold,' then of a waiter holding soup and spilling it on the wall. By the second day, most of this has disappeared, and he thinks only of a tray and a wall. The same occurs on the third day. On the fourth day, the image of a tray becomes very vague, and on the fifth day, the response is immediate without any association or imagery. The example from Miss Bro. is a type of great frequency. In learning 'kimono-stencil' for the first day, she pictures a design stenciled on the kimono. This picture recurs on the second day. On the third day, she thinks only of the words, design and stencil. On the fourth, fifth, and sixth days, the response, *stencil*, is immediate without any association or picture. These

examples show quite clearly how the associative connections between a stimulus and a response became shorter and shorter until finally the connection became immediate and direct, producing the shortest reaction time.

Summary

Associative aids disappear with practice and condition the rate of improvement. They greatly facilitate the formation of new responses but delay those which have been mechanized. Psychologically their function is apperceptive, that is, they connect the old with the new terms and then disappear. Neurologically they seem to provide indirect conductive units of low resistance between newly stimulated neurones and serve to connect them directly by the shortest paths.

IV

We may think of this experiment as an experiment in the transfer of training in which the regular tests for the six days constituted the training series, and the *B*, *D*, and *U* tests as the test series by which the spread of improvement from the training series may be measured. Although there were no tests before training, we may assume that in the average the L.T'.s for the *B*, *D*, and *U* tests would have been the same as the L.T.'s of the regular tests on the first day. We may measure the spread of improvement in one way by the difference between the L.T.'s of the training and test series, and in another way by the number of correct responses without prompting in the *B*, *D*, and *U* tests. Since the former measure mixes up the pairs in which there was facilitation with those in which there was interference, we had better take the latter measure, for it keeps these two sorts separate. In the *B* tests the pairs were learned backwards, *i. e.*, the subjects named the first word as a response to the second word as a stimulus. The reaction time in the average is almost twice as long as for the regular tests on the sixth day. The frequency of associative aids is from two to six times as great. Or in comparison to the regular tests, the associative frequency as well as the L.T.'s in the *B* test compare favorably to what they were on the third day of the regular tests.

This means that learning a given pair of words *forwards* completely for *six* days will reduce the time for learning it *backwards* as much as three days of practice in the forward direction. Learning forwards then helps learning backwards. The reappearance of associative aids in the latter makes it probable that an association which aids learning in one direction also aids it in another. This is more clearly shown in the *D* and *U* tests, in which the influence of associative aids was studied more carefully. Table VIII shows that the L.T.'s for the *SB* and *RC* series are higher in the *D* and *U* tests than in the regular tests for the first day. According to this measure, although these pairs were learned in the same order every day for six days, the learners were yet unable to name the first words in either an *up* or *down* order. There were, however, some correct responses in these tests, averages of 3.55 and 4.16 in the *D* test for *SB* and *RC* series respectively, and averages of 2.70 and 2.78 in the *U* test for the same series. That is, learning the pairs forwards completely each day for six days enabled the learner to name little over one third of the first words of the pairs in a *down* order and a little less than one fourth of them in an *up* order. But the learning of the other words was so much longer than the average that time was lost in the *D* and *U* orders because of the six complete learnings in the forward direction. Learning the pairs in one connection therefore in some respects produced very great interference with learning them in another connection. But it also greatly facilitated the learning of some of them. The *WS* and *WP* series do not show this interference in the average L.T.'s. Their L.T.'s in the *D* and *U* tests are, however, higher than the average L.T.'s on the second day of the regular tests, but less than those on the first day, showing very little profit by the six complete learnings in the forward direction. The number of correct responses, however, is 4.86 for the *U* test in the *WS* series and 2.50 for the *D* test in the *WP* series. These successes can only be accounted for by assuming that the training gained by the six days' practice on the pairs in the forward direction was transferred to learning them in both the *up* and the *down* orders. The question now is, how shall

we explain this transfer? The answer again is found in the influence of associative aids. This was determined by calculating, on the one hand, the per cent. of the correct responses that were made possible or rather accompanied by a revival of some association established in the training series, and on the other hand, the per cent. of such successes due to guesses or perseveration. Table XI shows the results of the calculation for the respective series.

TABLE XI

SB						*RC*						*WS*			*WP*		
D			*U*			*D*			*U*			*U*			*D*		
NC	% *AA*	% *PSV*	*NC*	% *AA*	% *PSV*	*NC*	% *AA*	% *PSV*	*NC*	% *AA*	% *PSV*	*NC*	% *AA*	% *PSV*	*NC*	% *AA*	% *PSV*
45	91.1	6.6	29	79.3	6.9	73	87.6	4.1	18	94.4	5.6	112	83.9	5.3	80	85.0	6.3
N = 117			*N* = 99			*N* = 144			*N* = 72			*N* = 216			*N* = 252		

NC = the number of correct responses in the *D* or *U* tests.
%*AA* = % of these accompanied by associative aids.
%*PSV* = % of these due to guesses or perseveration.

It will be seen that from 84 to 91 per cent. of these successes are accompanied by a revival of former associations, from 4 to 7 per cent. of them appear to be mere guesses, and the small remainder are unexplained. The fact that such a high percentage of correct responses are preceded by associative aids suggests that the successes are possible through them. This will be made more evident by reproducing some examples from the records which are worthy of careful study. The following points should be noticed: (1) The rare frequency of successes which cannot be explained by a previously established association. (2) The absence or confusion of such associations in cases of failure. (3) The high frequency of successes connected with associations of order and position. (4) The absence of direct connections between stimulus and response. (5) The connection of the latter by a roundabout previously established associative path which was reexcited either in the same direction in which it was established or in

a reverse direction. (6) The formation of new associations where a success was not obtained without prompting.

In the examples below the stimuli are printed along the margin at the left in the order in which they were given. In the *D* test, the subject was asked to name the first word of the pair next to 'sauce,' mistake,' etc. The responses and the times are given opposite the pair containing the correct response. Immediately below this are given the associations which the subject had formed with these responses, the connection being indicated by the corresponding numbers. In the *U* test the subject was asked to name the first word of the pair before 'space,' 'kimono,' etc. The reader must bear in mind that in this test such terms as before, after, first, and last, when used by the subject have just the reverse meaning from that which would be indicated by their position on the page. This is because the stimuli in this paper are printed in the reverse order from that in which they were in the original sheets.

Miss Bov. April 22, 4–5 P.M. *D* test.

Sauce balloon:	
Mistake clean:	1. Elephant 3.4″. 2. Mistake 1.4″.
	2. Prompting reminded me that mistake follows sauce.
Elephant steeple:	1. Elephant 1.8″.
	1. If elephant is not second, it is third.
Simmer tarry:	1. Tomato 11.0″. 2. Simmer 3.6″.
	2. Remembered prompting.
Ring kitten:	1. No 6.6″. 2. Galley 1.4″. 3. Kitten 1.6″.
	3. Remembered prompting.
Turkey among:	1. Turkey 2.2″.
	1. Thought ring, kitten, among, turkey.
Chest muffin:	1. Chest 1.8″.
	1. Thought turkey, among, muffin, chest.
Galley time:	2. Kimono 10.6″. 2. Galley 2.2″.
	2. Remembered prompting.
Kimono stencil:	1. Kimono 2.2″.
	1. Kimono is next to last.
Space betide:	1. Space 1.2″.
	1. Space is last.

1. Miss Ber. 4–5 P.M. *D* test.

Sauce balloon:	
Elephant steeple:	1. Elephant 8.3″.
	1. Thought sauce-balloon steeple-elephant.

Ring kitten: 1. No 10.3″. 2. Ring 2.0″.
1. Thought of muffin-chest stencil kimono; these are not right, no.
2. Nothing.

Chest muffin: 1. Chest 5.0″.
3. Thought ring, muffin-chest.

Kimono stencil: 1. Kimono 2.6″.
1. Thought chest-muffin, stencil-kimono.

Mistake clean: 1. Simmer 4.0″. 2. Mistake 5.4″.
1. Thought kimono-stencil, tarry-simmer.
2. Remembered prompting. Saw objects for kimono-stencil but words only for mistake-clean.

Simmer tarry: 1. Simmer 2.0″.
1. Thought mistake-clean-simmer.

Turkey among: 1. Turkey 4.0″.
Saw simmer-tarry, among-turkey in order.

Galley time: 1. No 17.0″. 2. Galley 2.0″.
1. Saw the words turkey-among and time, but could not see galley.
2. Remembered prompting.

Space betide: 1. Space 2.3″.
1. Saw the words galley, time, betide, space.

Miss Wal. April 20, 3–4 P.M. *D* test.
Radish coffee:

Serve hold: 1. Serve 6.1″.
1. Serve hold is the second pair.

Pork cocoa: 1. Pork 2.2″.
1. Pork cocoa is next to serve hold.

Cheese tomato: 1. Cheese 2.3″.
1. Guessed it.

Speak weigh: 1. Speak 1.2″.
1. Had these arranged like this: First there was something to eat, and then there wasn't, then there was again, and then there wasn't again.

Ribbon banana: 1. 1. Celery 4.2″. 2. Being 8.6″. 3. Ribbon 1.0″.
3. Nothing.

Being credit: 1. Celery 2.4″. 2. Being 1.4″.
2. Guessed it.

Miss Bro. 3–4 P.M., June 1. *D* test.
Sauce balloon:

Elephant steeple: 1. No 16.2″. 2. Elephant 9.8″.
1. Only balloon.
2. Only elephant.

Ring kitten: 1. Ring 2.8″.
1. Thought it belonged here somewhere.

Chest muffin: 1. Turkey 3.0″. 2. Tarry 10.4″. 3. No 30.8″.
1. Turkey the only one in the list that I could think of.
2. Only simmer-tarry.
3. First didn't know whether elephant came before or after, concluded it came before, but could not think what was right.
4. Thought of elephant all the time, then thought it was something a kitten could eat, chest-muffin.

Galley time: 1. Galley 1.0″.
1. Guessed at it.

Kimono stencil: 1. No 7.5″. 2. Kimono 1.8″.
1. Nothing.
2. Guessed at it.

Miss Rie. 2–3 P.M. *U* test.
Geigen fiddle:

Citrone lemon: 1. Citrone 2.0″.
1. Thought of the last and the next to the last pairs.

Besonder particular: 1. Besonder 1.6″.
1. Thought citrone-lemon, besonder-particular, and knew that besonder came before citrone.

Angriff attack: 1. Angriff 1.7″.
1. Thought besonder-particular, Angriff-attack.

Ereignen happen: 1. Ereignen 1.7″.
1. Thought Angriff-attack; ereignen-happen.

Dach roof: 1. Körper 2.0″. 2. Dach 5.6″.
2. Thought ereignen-happen; dach-roof.

Anregen stimulate: 1. Stimulate 6.2″. 2. No 18.0″. 3. Anregen 1.2″.
3. Remembered prompting.

Körper body: 1. Körper 1.6″.
1. If Körper is not before eregnen, it is before anregen.

Beharren persevere: 1. Beharren 2.2″.
1. Thought Körper-body, persevere-beharren.

Wipfel-summit: 1. Wipfel: 1.7″.
1. Thought beharren-persevere, summit-wipfel, and that wipfel and beharren are the first and second words.

Mr. Kin. 5–6 P.M. *U* test.
Soup wall:

Troll blast: 1. No 6.0″. Troll 1.6″.
2. Remembered from prompting that they were the last and the next to the last pairs.

Celery wafer: 1. No 3.3″. 2. No 5.5″. 3. Lettuce 8.0″. 4. Celery 5.4″.
4. Saw celery and remembered prompting.

Being credit: 1. No 3.2″. 2. Being 2.7″.
2. Remembered prompting.

Ribbon banana: 1. No. 1.6″. 2. Lettuce 3.0″. 3. No 6.2″. 4. Ribbon 3″.
4. Remembered prompting.

Speak weigh:	1. No 4.8″. 2. No 2.4″. 3. Cheese 10.2″. 4. Speak 2.0″. 4. Remembered prompting. A fellow selling ribbon in a dry goods department and then saw Dept. filled with groceries, and saw a cheese on the counter, and beside the cheese there was a ham, and then I saw a girl from the refreshment department serving radishes.
Cheese tomato:	1. No 1.6″. 2. Cheese 2.3″.
Pork cocoa:	1. No 1.6″. 2. Pork 2.0″.
Serve hold:	1. No 1.8″. 2. serve 2.8″.
Radish coffee:	1. No 1.4″. Radish 1.4″.

Mt. T. 8–9 P.M., May 24. *U* test.
Geigen fiddle:

Besonder particular:	1. Ereignen 2.8″. 2. Besonder 1.0″. 1. Thought of Angriff and then ereignen. 2. Nothing.
Ereignen happen:	1. Ereignen 1.4″. 1. Nothing.
Anregen stimulate:	1. No 29.0″. 2. No 22.4″. 3. Anregen 3.6″. 1. Thought of stimulate but could not find the German. 2. Same. 3. Remembered prompting.
Beharren persevere:	1. Beharren 14.6″. 1. Thought of anregen-stimulate, and that persevere is before stimulate and goes with beharren.
Citrone lemon:	1. Angriff 17.0″. 2. Citrone 3.0″. 1. Had an image of the particulars of two words. Angriff is before beharren. There is something in between, but I could not find it. 2. Nothing.
Angriff attack:	1. Angriff 1.6″. 1. Nothing, knew from previous associations.
Dach roof:	1. Dach 6.6″. 1. First a blank, then I recalled that something was before. I started at the top of the list, found the place for each word, and when I came to this place, Dach occurred to me.
Körper body:	1. Körper 2.0″. 1. Suggested by previous association.
Wipfel summit:	1. Wipfel 2.8″. 1. Wipfel is first and Körper second.

The first example is from the *D* test with Miss Bov. E. pronounced the first word of the first pair, 'sauce.' The proper response is 'mistake,' but the actual one was 'elephant' with a reaction time of 3.4 seconds. After prompting, E.

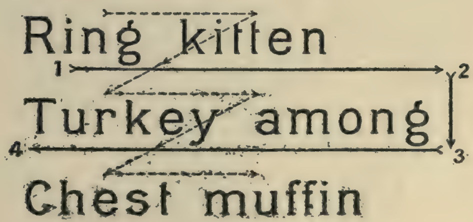
gave 'mistake' as a stimulus and obtained 'elephant,' the correct response. Subject's association was that if elephant did not come second, it came third. After E. went through the list in this way, he gave those stimuli again which were not correctly responded to the first time. He began with 'sauce' and obtained the correct response, 'mistake,' in 1.4 seconds. The previous prompting reminded the subject of the correct position of mistake. Miss Bov. has five successes in this test. Three of these are due to *associations of position.* Thus elephant is the third word, kimono is the next to the last, and space is last. The other two successes are due to *associations of order.* For example when she was asked to name the first word of the pair after 'turkey' she thought the words 'turkey,' 'among,' 'muffin,' 'chest,' and then said 'chest.'

The above two types of association explain most of the successes in the *D* tests. Words near or at the ends were associated with absolute position and those in the middle with the order of learning. There are a few instances, however, in which the order of the pairs was connected with meaningful associations. For example, Miss Wal. had the first five pairs of the *RC* series connected with the story. "First there was something to eat, and then there wasn't again, and then there was again, and then there wasn't again." Some had scarcely any association of position or order, and the *D* test presented a case of learning a new series. The first record from Miss Bro. is a case in point.

The *U* tests show only one important difference in the associations by which the successes were obtained, namely, that they are worked out in the reverse order from that followed in learning. For example, when Mr. T. was told: "Name the first word of the pair that comes before 'anregen'" he replied correctly, 'beharren,' with a reaction time of 14.6″. The association was: "I thought of 'anregen-stimulate,' and that 'persevere' is before 'stimulate' and goes with 'beharren.'" The associations of absolute position are just as frequent as in the *D* test. A few subjects showed pronounced visual imagery in this test, *e. g.*, when Miss Ber. was asked to name

the word before 'Angriff,' she reported that she saw a 'Dach' above it and the third from the top. The second record from Miss Bro. is an example of a subject who had few successes in the *U* test, and who had to form a set of new associations in order to learn the pairs in this order.

Although not all of the pairs fall within the category of order and position, the latter are the only ones of importance. The diagram given below will serve as a basis for discussing these associations. A success in the *D* test depended on a definite association both from 1 to 4. A success in the *U* test depended on the ability to travel the path from 4 to 1. A success in the *B* test depended on the ability to travel the path from 2 to 1. Most frequently the path extended only from 1 to 2. Sometimes it extended in addition to 3, and the subject named the word at 3 but was unable to think of the word at 4. Sometimes the path extended only between 2 and 3 and between 3 and 4, enabling the learner to travel from 4 to 3 but no farther. It is important to mention these incomplete paths because they explain the failures in the *D* and *U* tests just as the complete paths explain the successes.



The great frequency of the associative path designated by the solid lines gives rise to the question why the subject did not associate the pairs in the order of presentation as is indicated by the broken lines. At first thought it would appear that the latter would also be the order of the learning. The solid line arrows, however, indicate the direction in which the subject *thought* them and *reacted* to and with them. For the subject to think: "After 'kitten,' I say 'among' to 'turkey' and then 'muffin' to 'chest'" was a simpler order

then to think: "After 'kitten' comes 'turkey' to which I say 'among' and then comes 'chest' to which I say 'muffin'." There are cases of the latter order, but they are much rarer than the former. It appears here that the order of *thinking* and *reacting* is much more important for establishing an associative path than the order of presentation. In spite of the fact that E. said the first words in the same order many times, day after day, the learner after all did not establish any association in that order nor achieve any successes in the *D* and *U* tests by such associations. The order of *listening* counted for little in comparison to the order of *responding.*

This emphasizes the importance of selective activity on the part of the learner in the formation of associations and leads me to a digression on the law of contiguity, which appears to be at variance with the facts of experience. The mere togetherness of two presentations in space or time is no proof that they will be associated. If it were, there should have been few failures in the *D* and *U* tests, for the frequency of the connection 1–2, was hardly any greater than that of the connection, 1–2–4, in the above diagram. Yet the subject, on the sixth day, remembered the former connection in over 95 per cent. of the cases, but in the latter he failed in 66 per cent. of the cases. This is striking proof that the frequency of contiguous presentations is not alone sufficient to establish an association between them. Some factors determining this situation were: The subject was interested in the connection, 1–2, it was exercised, and its exercise was satisfactory. The subject was not interested in the connection 1–2–4, and it was not exercised although it was objectively presented. But of these, the most important factor is probably exercise. From a neurological standpoint, it is not at all certain that the presentation of an object to a learner stimulates his sense organs, and if it does, it is again not certain that it stimulates a complete sensory-motor arc. Even if the latter does occur, the exercise of the bond in question may be so feeble as to leave no after-effect. But whatever the explanation of the failure may be, it at least appears from the results of this

experiment that only those experiences to which we definitely react are associated. The following formula of the law of contiguity is therefore suggested: If a learner reacts to *A* and *B* together in some experience, and if later he again responds to one of them, the former reaction to both of them tends to follow; or stated more simply, if a learner reacts with *A* to *B* and if later *B* recurs, *A* tends to follow. In this connection I am reminded of Hunter's revision of the law of association in the Psychological Review, May 1917. While I agree that the second term of an association may be either sensory or ideational, I do not think that the failure to recognize this is the fundamental weakness of the traditional formulation. Its weakness is rather that it emphasizes the togetherness of an experience that is important in the establishing of an association. But as we have seen, this is of little importance in comparison to a positive reaction to the experience for the purpose in question.

Coming now to the main question at issue in these *D* and *U* tests, the explanation of the transfer of training, it appears from the above results that learning one thing helps to learn another only in so far as the associations which were established in the first can be made use of in the second. It is clear from the results of this experiment that learning things in one direction helps to learn them in another, that is, when two or more objects are learned well enough in one direction, it is possible at another time to recall these objects in a new order; but, if so, the objects in question must have been connected by some *actual* association *between* them. If association explains the transfer of training, it also should explain the variability in the amount of transfer between one individual and another. That this is the case is evident from Table XII., which shows that the number of correct responses in the *D* and *U* tests varies concomitantly with the number accompanied and preceded by associative aids. Now and then there is an exception which is easily explained by perseveration or a possible forgetting of the association in question. But the correspondence is so close that the main conclusion is inevitable.

TABLE XII

SB								RC								WS				WP			
D				U				D				U				U				D			
S	NC	NA	PSV	S	NC	NA	PSV	S	NC	NA	PSV	S	NC	NA	PSV	S	NC	NA	PSV	S	NC	NA	PSV
Joe	7	7		Fac	5	4		Fra	8	8		Ber	4	3	1	Jen	9	9		Gib	6	5	
Ber	6	6		Jen	4	4	1	Sin	7	7		Ham	4	4		Tra	8	5		Pfe	6	3	
Gre	6	5	1	Tra	4	2		Wol	7	5		Joe	4	4		Ber	7	7		Bov	5	4	
Bov	5	5		Wal	4	4		Gib	6	5		Pan	4	4		Fac	7	5		Gre	5	4	1
Kel	4	4		Dav	2	2		Jen	6	5	1	Hil	2	2		Joe	7	7		Sul	5	5	
Ham	3	3		Hof	2	1		Pfe	6	6		Kel	0	0		Pfe	7	3	1	Bro	4	2	2
Hil	3	3		Huf	2	1	1	Tra	6	6		Rip	0	0		Rie	7	7		Huf	4	3	
Pip	3	2	1	Sul	2	2		Dew	5	4		Wic	0	0		Bov	6	4	1	Pan	4	4	
Bro	2	1	1	Swa	2	2		Eva	5	4						Eva	6	6		Bor	3	3	
Kin	2	1		Pfe	1	1		Rie	5	4						Pan	6	5	1	Dav	3	3	
Pan	2	2		Rie	0	0		Teh	4	4						Teh	6	6		Eva	3	3	
Woo	2	2						Bor	2	0						Bor	5	5		Fac	3	2	
Wic	0	0						Hof	2	2						Bro	5	4	1	Huf	3	3	
								Huf	2	2						Ham	4	3	1	Kel	3	3	
								Woo	2	2						Hil	4	4		Teh	3	3	
								Swa	0	0						Huf	4	3		Wall	3	2	1
																Kel	4	4		Woo	3	3	
																Tra	4	3		Ber	2	2	
																Woo	4	3		Ham	2	2	
																Gre	2	1	1	Hof	2	2	
																Kin	0	0		Joe	2	2	
																Rip	0	0		Wic	2	2	
																Swa	0	0		Hil	1	0	1
																Wic	0	0		Pip	1	1	
																				Swa	1	1	
																				Tra	1	1	
																				Kin	0	0	
																				Rie	0	0	

S = Subject.
NC = No. of correct responses.
NA = No. of correct responses having associative aids.
PSV = No. of correct responses apparently due to perseveration.

The training and the tests of this experiment are not sufficiently varied to prove conclusively that associative bonds are the only factors which determine the extent, limit, and possibility of the spread of improvement. But we can test the validity of this theory to a certain extent by examining how far it appears to be adequate for explaining the results obtained by other investigations in this field.

If the transfer of training takes place by means of common associative bonds, then the explanation of this phenomenon is simply a case of bringing it under the laws of association. Theoretically the problem with the experiments

on the transfer of training is to find in them illustrations of the laws of association. If the mind is primarily a psychophysical mechanism for reaction, then common associative bonds may occur either through identity and similarity in the stimulus or through identity or similarity in the response. The stimulus may be either sensory or conceptual and the response may either be conceptual or overt. Training may then be transferred from one performance to another when the two have (1) common sensory stimuli; (2) common conceptual stimuli; (3) common overt responses; (4) common conceptual responses.

It may be difficult to bring all the reported cases of the transfer of training under the above categories, but it appears that most of the authentic cases of transfer or of its failure can be so classified.

The following cases of positive transfer may be noticed: When there is transfer of training from estimating areas 10–100 sq. cm. in size to estimating areas 10–250 sq. cm. in size in proportion as the size of the latter approach the size of the former, there appears to be a common response because of similarity in size. When training in cancelling words with e and s is transferred to cancelling words with e and r, or s and p more than it is to cancelling misspelled words or to cancelling capital A's mixed up with a number of other capitals, there is again a common response to the extent that there is a common sensory stimulus in the letters or small geometrical forms in a certain order.[1] When practice in typewriting certain visual stimuli in one order improves the ability to typewrite them in another order, there are common sensory stimuli in small letters appearing at the same place and in a certain order and in the feel of the keys. There are also common responses in the eye movement from copy to keys and in the finger movements to the particular visual stimuli.[2] Transfer from improvement in discriminating intensities of red to discriminating intensities of mixtures of yellow and

[1] Thorndike and Woodworth, Psychol. Rev., 1901, 8, 247–261; 384–395; 553–564.

[2] Bair, J. H., Psychol. Monog., No. 19, 1902.

green, intensities of orange, and differences in pitch, is a case of common response by careful observation of small differences. That there is more transfer between small differences within the same modality than there is between those in different modalities again shows the influence of the extent of similarity in the stimulus. Transfer from training in reproduction of one order of four intensities of sound after hearing them in another order to similarly arranged reproductions of the order of four grays, four tones, nine grays, nine tones, nine geometrical figures, two stanzas, and nine numbers—involves common sensory stimuli in separating out order and in the arrangement of stimuli; a common conceptual stimulus in 'Remember one order while perceiving another'; a common conceptual response in associating the stimuli in a certain order; and a common overt response in naming them in a certain order.[1] The order in which the test series were named above shows the extent of the improvement from most to least, and this again shows the importance of the extent of the similarity in the stimulus in number, quantity, or span. Transfer of training in descriptive geometry in amount from most to least to ability to name the number of lines necessary to construct various geometrical objects, to state the number of strokes necessary to write certain words in the straight-line alphabet, to divide mentally three-place numbers by one-place numbers, to state the number of one-inch cubes in a three-inch painted cube which have 0, 1, 2, and 3 painted sides respectively, and to write as many words as possible from the letters in 'material,' is a case that invokes a common sensory stimulus in spatial elements; a common conceptual stimulus in 'Separate them out, hold them apart and put them together'; and a common conceptual response in the mental manipulation and association of spatial elements.[2] The influence of a common stimulus is again evident here because there is more transfer to geometrical objects than to quasi-geometrical objects and more to the latter than

[1] Fracker, G. C., Psychol. Monog., No. 38, 1908, 56–102.

[2] Rugg, H., 'The Experimental Determination of Mental Discipline in School Studies,' 1916.

to non-geometrical objects. Transfer of training from learning nonsense syllables by one method to learning them by another method involves common sensory stimuli in similar material; a common conceptual stimulus in 'Learn them as soon as possible'; and common conceptual responses in the form of association and reproduction.[1] Transfer from neatness in one school subject to neatness in another school subject is a case of responding to the common[2] conceptual stimulus 'Be neat.'

Certain cases of failure in transfer of training are equally significant for the theory stated above. The following may be noticed.

Training in card sorting does not improve ability to typewrite. Training in estimating areas 10–100 sq. cm. does not improve ability to estimate similar areas over 200 sq. cm. in size. Training in estimating lines .5 to 1.5 inches long does not improve ability to estimate objects from 2.5 to 8.75 inches long when the latter consist of such things as envelopes, brushes, and wrenches. Training in estimating four intensities of sound does not improve ability to estimate the extent of arm movement. Training in cancelling parts of speech does not improve ability to cancel words having the letters e and t.[3] Training in memorizing 'Paradise Lost' does not improve the memory for Hugo's verse.[4] Training in memorizing nonsense syllables does not increase the memory-span for letters, numbers, nonsense syllables, disconnected words, Latin-English vocabularies, poetry, and prose; nor the ability to memorize completely meaningless visual characters, Latin-English vocabularies, and passages of poetry and of prose.[5] Training in memorizing prose substance does not improve the ability to memorize dates, nonsense syllables, poetry, points on a map, dictation, letters, and names. Training in memorizing tables does not improve ability to memorize

[1] Reed, H. B., 'Repetition of Ebert and Meumann's Practice Experiment in Memory,' J. of Exp. Psychol., 1917, 2, p. 315.

[2] Reudiger, W. C., *Educ. Rev.*, 1908, 36, p. 364.

[3] Kline, L. W., *J. of Educ. Psych.*, May, 1914.

[4] James, W., 'Principles of Psychology,' 1890, Vol. 2, p. 67.

[5] Reed, H. B., J. of Exp. Psychol., 1917, 2, 315ff.

dates, poetry, prose, prose substance, dictation, letters, and names. Memorizing poetry does not improve ability to memorize dates, poetry of another sort, prose, prose substance, points on a map, dictation, letters, and names.[1]

I have also examined the results of experiments on the transfer of training by Bagley and Squire,[2] Briggs,[3] Burnet,[4] Coover,[5] Dallenbach,[6] W. F. Dearborn,[7] Foster,[8] Hewins,[9] Judd,[10] Ruger,[11] Scholkow and Judd,[12] Wallin,[13] Whipple,[14] and Winch,[15] but have not been able to make them the basis of a theoretical discussion because they were too indefinite, irregular, or complicated to bring within a consistent rule. However, no theory that is at all specific can explain all cases of reported transfer. The psychological factors in the cases of positive transfer described above were pointed out. In the cases of negative or zero transfer it is difficult to find common sensory stimuli, and common conceptual stimuli that may exist are too general to be effective. It is also difficult to find common conceptual or associative responses. But some cases of positive transfer are equally baffling; for example, training in memorizing poetry has been found to improve the ability to locate points on a map and to memorize nonsense syllables, but interfered with the ability to learn poetry of another sort, prose, or prose substance. Memorizing tables improved the ability to locate points in a circle and to learn nonsense syllables, but interfered with the ability

[1] Sleight, W. G., *British J. of Psych.*, 1911, **4**, 386–457.

[2] Bagley, W. C., 'Educational Values,' 1905, p. 188 ff.

[3] *Teachers College Record*, Sept., 1913.

[4] 'Formal Discipline,' Columbia University Contributions to Education, 1907.

[5] Psychol. Monog., No. 87, 1916.

[6] *J. of Educ. Psychol.*, 1914, **5**.

[7] *J. of Educ. Psychol.*, 1911, **1**, 386–457.

[8] *J. of Educ. Psychol.*, 1911, **2**, 11–21.

[9] 'The Doctrine of Formal Discipline in the Light of Experimental Investigation,' 1916.

[10] Psychol. Rev., 1902, **9**, 27–39.

[11] 'Psychology of Efficiency,' *Archives of Psychology*, No. 15.

[12] *Educ. Review*, 1908, **36**, 28–42.

[13] 'Spelling Efficiency in its relation to age, sex, and grade,' *Educ. Psychol. Monographs*, 1911.

[14] *J. of Educ. Psychol.*, 1910, **1**, 249–262.

[15] *British J. of Psychology*, 1908, **2**, 284–293.

to learn dates, poetry, or prose. The difficulty with all these studies is that the associative processes were not investigated. If we knew what the common bonds of association had been in these cases of positive and negative transfer, we probably should have the cue to their explanation. The correlation between observable stimuli and observable responses is too irregular to make a consistent principle inferable with certainty, but such regular correlations as there are point quite definitely to the solution of the problem in the laws of association. It is to be hoped that future investigators of this problem will more carefully examine the internal facts of transfer, *i. e.*, the common associative bonds.

The above theory does not contradict Thorndike's theory of identical elements. It simply makes it more specific and states that these identical elements consist of associations. The difficulty with the theory of identical elements is the indefiniteness of its meaning. At first it meant principally identity of content but later was extended to identity of procedure and identity of neurological process, but these again are terms of no specific meaning. The wide latitude of the meaning of identical elements is clearly shown by Coover who, both by observation and by introspection, found no less than "40 varieties" of the identical element between one process and another. But the scientific problem with them is to show in verifiable, quantitative evidence which of those forty factors are important and which are not important. That an element is common between two processes tells us nothing about how much one process influences another process unless there is concomitant variation between the amount of this influence and the amount of the common factor.

An examination of Coover's results from this standpoint brings disappointment. His results were recently reported in a three hundred-page monograph. Space prevents me from reviewing it, but if the reader will turn to the sections on "Card-sorting" and "Typewriter reaction," pages 118–131, he will get an idea of Coover's method. He finds no less than "twenty varieties" of the card-sorting process.

When he gives the results on the amount of transfer by various subjects in visual apprehension, sound reaction, and mnemonic devices upon the card-sorting reaction, he finds a residual gain of 10 per cent. on the part of the eight trained subjects. But there were only two controls and these gained as much as two of the trained, and a good deal more than one of them. How then can he be certain that the 10 per cent. greater gain by the other five subjects is not a matter of accidental variation? As an explanation of this very uncertain transfer, he offers the following factors: heightened sensitivity for visual impressions, heightened reproductivity of imagery, coördination of part-processes, exercise of continuous attention and reproduction of memorial elements and habituation to distraction. But he offers not an iota of quantitative evidence that any of these factors are important for improvement or for transfer. If these hypothetical factors are of any importance in either connection, we should have some scientific and objective evidence as a ground for our judgment, *e. g.*, some constant connection or concomitant variation ought to be shown between any of these factors and improvement or transfer. Unless an investigator can produce such evidence, his results only multiply the difficulties of the problem in question.

Summary

In this experiment evidence that learning things in one order helps to learn them in different orders is considered proof of transfer of training. Such transfer takes place through the use of associative bonds common to the old and new orders. Such a use is essentially a case of thinking through old associations in new directions.

The evidence for the theory that transfer of training must be explained by common associative bonds is contained in Tables XI and XII. The former shows that from 84 to 94 per cent. of the cases of transfer are accompanied by associative aids and the latter shows a concomitant variation between the number of cases of transfer and the number of associative aids. On these grounds a causal relation is assumed.

Further evidence for the theory is furnished by an examination of the literature of the clearest cases of transfer. If associative bonds are the media of transfer, then the explanation of these cases is simply a problem of bringing them under the laws of association. This would mean that training may be transferred from one performance to another when the two have (1) common sensory stimuli; (2) common conceptual stimuli; (3) common overt responses; (4) common conceptual responses. An attempt to explain the clearest cases of transfer by these principles appears reasonably successful.

The above theory does not contradict Thorndike's theory of identical elements, but simply gives it a specific meaning. It also insists that identical elements cannot be assumed to be media of transfer unless the quantitative proof for them is clear.

Incidentally the experiment demands a reformulation of the law of contiguity. The togetherness of objects in experience is not a sufficient condition of association unless it is accompanied by active attention.

INTELLIGENCE AS ESTIMATED FROM PHOTOGRAPHS

BY RUDOLF PINTNER

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There is a widespread belief that it is a relatively simple thing to estimate a person's intelligence after a perfunctory conversation with him or even merely by looking at him. One hears again and again the expressions, 'he looks bright,' and 'he looks stupid.' Such judgments of intelligence are most often made in regard to children. Teachers have a very firm belief in their own ability to judge the intelligence of their pupils, in spite of the fact that correlations of their estimates with the results of mental examinations have been shown to be decidedly low. Physicians are also very prone to make snap-judgments of people and, because of their personal contact with a large number of people of all classes, they come to believe tremendously in their own ability in this direction. The physician, of course, is not primarily judging intelligence, in the narrow sphere of the psychologist's definition. He is judging rather that larger and vaguer thing we call 'character.' Neither the physician, however, nor the teacher hesitate for a moment in judging the intelligence of individuals, if challenged. Indeed, they do not appear to think it more difficult to judge intelligence pure and simple than to judge 'character.' There is, however, a group of physicians who are daily passing judgment in regard to the intelligence of individuals. This group includes the psychiatrists, the physicians in institutions for the feeble-minded and some others who happen by chance to be repeatedly called in to a probate court to pass upon cases of suspected insanity and feeble-mindedness. Most of these believe thoroughly that that magical thing known as 'experience' has endowed them with the gift of estimating the intelligence of a case after a brief interview, in which appear certain

stock questions about the President of the United States, and so forth. Most of them scorn any assistance that mental tests may give them.

Because there are many practical difficulties in obtaining the judgments of a number of individuals in regard to the intelligence of the same group of children, we have substituted photographs for the living child. This substitution, of course, makes the problem more difficult. It is much harder to make a judgment of intelligence about a photograph than about the living child. Whether the judgments in the latter case would be more accurate has not been investigated. The presumption is, of course, that they would be, because the movements and the language of the child give us added data from which to draw a conclusion as to his intelligence.

In this investigation twelve photographs of children of varying ages were used. All of these children had been tested by the writer on the Yerkes-Bridges Scale. The photographs are shown on the following pages, and under each child the chronological age, the mental age, and the coefficient of mental ability are given. The pictures of children are arranged in the order of intelligence as determined by the C.M.A., reading from left to right and downwards. Of the children Nos. 1 and 2 would be classed as very bright; No. 3 as bright; Nos. 4, 5 and 6 as normal; Nos. 7, 8 and 9 as backward; and Nos. 10, 11 and 12 as feeble-minded. In fact the three feeble-minded children are at present inmates of a feeble-minded institution.

The observers were asked to arrange the twelve photographs in the order of their intelligence, beginning with the most intelligent child. The ranking according to the observer was correlated with the ranking according to the mental examination, the formula

$$\rho = 1 - \frac{6 \Sigma D^2}{n (n^2 - 1)}$$

being used.

The observers have been grouped into four classes, namely, 9 physicians, 15 psychologists, 11 miscellaneous, 11 students and 17 teachers. The group of physicians was made up of two general practitioners, one tuberculosis special-

ist, one skin specialist, one anæsthetist, one physical director of a large public-school system, and three physicians employed in institutions for feeble-minded. Of the group of 15 psychologists, all except one had had experience in giving mental tests. Seven were devoting most of their time to clinical or abnormal psychology. Four were graduate students or assistants specializing in mental tests. Of the 11 miscellaneous people, three were business men, one was a married woman, and seven were stenographers. The group of 11 students was part of a class studying the problems of feeble-mindedness and the measurement of mentality. All of them had attended clinics throughout the semester, in which all types of defectives were shown, and many of the students had had some limited experience with mental testing. The group of 17 teachers was a summer-school class in mental measurements made up of teachers and superintendents. A few of these had had experience in the giving of mental tests. All our groups, therefore, with the exception of the miscellaneous group, had been brought face to face with the definite problem of the differences of intelligence that exist in children. The miscellaneous group had never had this problem brought to their attention and it is doubtful whether there existed in their minds any very definite idea as to the meaning of the term 'intelligence,' and no academic definition was given them before they were asked to arrange the photographs. Although several of the observers remarked on the difficulty of the experiment, none paused because of any difficulty about the meaning of intelligence. As a rule, very little hesitation was shown by the observers in making their decisions.

The correlations of the observers arranged according to these groups are given in Table I. Minus signs are inserted before negative coefficients; plus signs have been omitted. The coefficients are arranged in order of size. The first column gives the coefficients of the physicians, and is to be interpreted as follows: The first physician, who made the highest coefficient, had a coefficient of +.28; the next physician of +.25, and so on down to the negative coefficient of

−.21 of the last physician. The row, marked '*n*' at the bottom of the table gives the number of individuals in each group. The next row, marked 'Av. *r*,' gives the average of the coefficients for each group. The next row, marked

TABLE I

DISTRIBUTION OF COEFFICIENTS OF CORRELATION

	Physicians	Psychologists	Miscellaneous	Students	Teachers
	28	45	52	29	37
	25	41	39	27	34
	21	40	31	24	33
	19	38	14	15	27
	04	34	09	06	27
	−08	33	01	03	24
	−10	31	01	−06	15
	−14	27	01	−07	13
	−21	15	−19	−08	10
		13	−32	−24	08
		−01	−51	−30	08
		−02			06
		−09			−05
		−63			−07
					−26
					−28
					−29
n...........	9	15	11	11	17
Av. *r*........	+.05	+.18	+.05	+.03	+.09
Av. rank *r*....	+.11	+.31	+.06	+.03	+.17

Median *r* for all cases = + .10.

'Av. rank *r*,' gives the correlation between the ranking of the twelve photographs as determined by the whole group and the ranking as determined by the mental examination. The median coefficient for all the 63 observers is +.10.

An examination of the coefficients of each group is interesting. The highest coefficient among the physicians is made by one of the general practitioners, who had some interest in mental tests. The three institution physicians made coefficients of +.04, −.08 and −.10, or, on the average, lower than the average of the other six physicians. The average for all the physicians is +.05. It would seem, therefore, that this group of observers is not able to arrange the photographs according to intelligence. Their average arrangement is about what might be expected from a chance arrangement of the cards.

No. 1.	Chron. Age 4.	M. A. 5.7.	C. M. A. 1.93
No. 2.	Chron. Age 5.	M. A. 7.2.	C. M. A. 1.64
No. 3.	Chron. Age 10.	M. A. 12.	C. M. A. 1.21
No. 4.	Chron. Age 10.	M. A. 10.8.	C. M. A. 1.09
No. 5.	Chron. Age 7.	M. A. 6.8.	C. M. A. .97
No. 6.	Chron. Age 16.	M. A. 12.5.	C. M. A. .90

No. 7.	Chron. Age 10.	M. A. 9.	C. M. A. .88
No. 8.	Chron. Age 12.	M. A. 9.7.	C. M. A. .81
No. 9.	Chron. Age 12.	M. A. 10.	C. M. A. .80
No. 10.	Chron. Age 15.	M. A. 9.8.	C. M. A. .72
No. 11.	Chron. Age 12.	M. A. 8.6.	C. M. A. .72

Among the psychologists there are three coefficients above 40 and seven above 30. All of these are low, but they denote some measure of agreement. But, on the other hand, there are four negative coefficients and one, $r = -.63$, the largest negative coefficient in the whole table. This coefficient was made by a psychologist whose particular interest is abnormal psychology. The highest coefficient, $r = .45$, is also made by a psychologist whose chief interest is abnormal psychology and who has had varied experience in mental survey work. The seven psychologists who have had much experience in mental tests made coefficients of 45, 41, 38, 13, −02, −09, −63. The wide variation in these coefficients shows that experience in mental testing is no guarantee of ability to judge intelligence as shown in the photographs of people. Taking the psychologists as a group, we note that the average r is +.18. The average rank of the children obtained from all the psychologists' rankings gives a correlation of +.31 with the real arrangement. This coefficient is high enough to indicate a measure of agreement beyond mere chance. However, it takes the average ranking of 15 psychologists to obtain this correlation. If the large negative coefficient of −.63 be omitted, the average of the 14 remaining coefficients is +.22. If the observer who made this high negative coefficient had not been a clinical psychologist, a fair average for the clinical psychologists might have been obtained, but as it is, it would seem as if the clinical psychologist is just as likely to err in his judgment of intelligence by means of photographs as is any other individual.

In the miscellaneous group is to be found the highest single coefficient, namely +.52, and this was made by a stenographer. It would seem as if a business training in shorthand and typewriting is just as useful in training us to be good judges of the intelligence as expressed in people's photographs, as is a study of medicine or a Ph.D. in psychology. In this same group, besides the highest positive coefficient, there is also the second largest negative coefficient, $r = -.51$, made by a business man. The average of the coefficients is +.05, the same as the physicians' average.

There is nothing of note in the coefficients of the students. They range from +.24 to −.30, showing a very normal distribution. Their average is +.03, the lowest average for any of the groups.

The coefficients of the teachers' group range from +.37 to −.29 and also show a fairly normal distribution. The average of the coefficients is +.09.

Ten chance arrangements of the photographs were made and the rankings correlated with the ranking according to the mental tests. The average of the coefficients is −.08, and the coefficients range from +.43 to −.74. Except for the high negative coefficient of −.74, the chance coefficients are just about as good as those of the observers in general.

All the four groups show a very small average coefficient. That of the psychologists is the highest, but is not sufficiently high to denote any great degree of resemblance between the rankings. The median r for all the 63 individuals who performed the experiment is +.10. On the whole, therefore, we may say that it is impossible by means of photographs to rank children according to their intelligence. Some of the high correlations that were obtained are probably due to chance. This is suggested by the fact that the psychologist who made a coefficient of +.45 only succeeded in making a coefficient of +.28 when ranking the photographs a second time about six months after the first trial. That his judgment of the children remained fairly constant is shown by a coefficient of +.76 between his two arrangements.

It is interesting to note that there was a fairly close agreement between the average ranking of the children for the four groups of observers. The average rank of each of the twelve children for each group of observers was calculated, and these rankings were correlated with the final ranking obtained from all the 63 observers. The coefficients for the five groups of observers are .91, .94, .84, .89 and .92 respectively. This shows a fairly close agreement among the judgments of the observers as a whole. The correlation of the ranking as determined by all the observers with the true ranking is +.16.

Table II. shows the rank according to the mental tests, the rank according to the 63 observers, obtained from the average rank, which is given in the next column, and finally the average deviation. The average deviation gives us a measure of the difference of opinion among the observers as to the rank of the child. The smallest A.D. is 1.5 for child No. 3. This child is given first place more frequently than any other child, and there is the least difference of opinion

TABLE II

RANKING OF CHILDREN BY ALL OBSERVERS

Child No.	Rank	Av. Rank	A. D.
1.	4	5.2	2.3
2.	9.5	8.9	2.1
3.	1	2.7	1.5
4.	5	5.2	2.3
5.	11	9.4	1.7
6.	6	5.4	2.3
7.	3	4.9	2.2
8.	12	9.7	2.0
9.	7	5.9	2.4
10.	9.5	8.9	2.1
11.	2	3.3	2.3
12.	8	7.5	2.1

about him. Child No. 3 shows the thoughtful and serious aspect that is commonly associated with superior intelligence. The tests show him to rank third and he would be classed somewhat above normal, as a bright boy. The next two smallest average deviations are those of children Nos. 5 and 8, the two who receive the lowest average rank. The observers as a whole differ least as to which they consider the brightest child and which the two least intelligent. No. 5 was repeatedly called 'stupid' and often 'imbecilic,' and some observers pointed to the position of the hands as suggesting the picking of the fingers seen sometimes in idiots. No. 5 is, as a matter of fact, a perfectly normal child. No. 8 was called 'wild' and 'silly' and receives the lowest ranking according to our observers. By the mental tests she would be considered a backward, but not feeble-minded child. The largest average deviation is that of child No. 9. According to the tests he ranks ninth and would be classed as

backward but not feeble-minded. The average rank of the observers places him seventh. Many observers place him low because of his rather expressionless face, but the large average deviation is caused by a few observers who place him very high. Although this was never openly acknowledged, the writer suspects that in some of these cases the observers were influenced by his glasses and placed him high because of the intelligence conveyed by those useful adornments.

Comparing the rank obtained by the observers as a group in column two with the rank according to the mental tests in column one, we note that the greatest divergence occurs in child No. 11, who is ranked second by our observers. This child has the sparkling eyes and bright face that conforms so well with our popular conception of 'brightness,' and yet she is an inmate of a feeble-minded institution. The next greatest divergence is in child No. 2, who is ranked 9.5 by our observers. He is a very bright boy, according to the tests, and yet his expressionless face and rather awkward pose is interpreted as intellectual dullness by the observer. The tests and the observers both agree in placing child No. 6 about the middle of the group. There is also a close agreement with regard to child No. 10, whose fat expressionless face is taken to be indicative of a lack of intelligence. Child No. 4 is placed fifth by our observers, although what was regarded as a silly smile by some led them to rank him low.

It is impossible to analyze out of all the opinions of the observers any single factor or set of factors which seem to guide us in our judgments of intelligence when confronted with pictures. Some observers seem to look at nothing in particular and make their judgments in a hap-hazard fashion, while others will draw the most definite conclusions from slight hints suggested by the hands or face or clothes of the child, and in most cases it would seem as if such conclusions are as likely to be false as they are to be true. Some are greatly influenced by the pleasant appearance or smile, but for some the smile denotes intelligence and for others it denotes feeble-mindedness. The position of the hands is all-

important to one observer, and, if the feet had been showing on all the pictures, it is probable that they would have been of diagnostic value for some observer. Several observers confessed being greatly influenced by the resemblance of some of the photographs to children of their acquaintance, and it is doubtless true that everyone is influenced in this way, consciously or unconsciously.

It is the writer's opinion that the trivial things mentioned here are at work in the snap judgments that we make of people in everyday life, and that they are also at work, perhaps to a lesser degree, in the judgments of children made by teachers, physicians and psychologists, who do not use mental tests for purpose of mental diagnosis. The writer does not believe that the correlations would have been very different, if the actual children could have been used instead of the photographs. They would have been somewhat higher, if an interview of five minutes had been allowed with each child. They would only become very high when each observer makes use of some objective criterion or test.

THE GENESIS OF THE IMAGE

BY CURT ROSENOW

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In this year of grace, the doctrine that imagery arises at times when other more habitual psycho-physical processes are unable to deal with a problematic situation is scarcely in need of defense. Nor is it my purpose, in the present article, to rush to the aid of the victor, at any rate not without endeavoring to show that he is in need of assistance. For I wish to point out that a doctrine which is capable of being expressed only in terms as vague as the above is an article of faith rather than a scientific explanation. Unless we wish to call a day dream a problematic situation we must admit that, as a matter of fact, imagery arises in profusion at other times also, so that the appeal of the doctrine seems to be based less on factual verification than upon the way in which it fits in with the developmental, genetic point of view. If so, it would seem incumbent upon the defenders of the faith to state their case in intelligible fashion. If the doctrine does not describe the facts of adult life correctly, it should at least give us, as precisely and concretely as possible, the hypothetical conditions which would tend to make the genesis of the image appear if not the necessary result, at least a plausible and intelligible consequence of such conditions. And yet, when I let my mental gaze wander over the relevant literature of the past two decades, I am unable to find anything like such a description.

Take for example the way Angell states the case: "In seeking to detect the appearance—of imagery we must remember that from the outset of life organic activities are in progress and the sensory motor activities in particular are in full swing. Each sensory stimulus is producing movements, which in turn are productive of fresh sensations. It is out

from such a cycle of onward-moving coördinations as these that the image emerges; and—it must be that the image is called forth by some need of the organism which the processes that we have already described are incompetent to satisfy."[1] But the existence of *some need of the organism* is not, alas, a guarantee of its satisfaction. If I feel the desire to fly, that does not imply that I will at once proceed to sprout wings.[2]

Nor can it be said that Baldwin's account of the matter is any more successful. In attempting to account for the origin of memory he says, referring to the neural associative machinery: "Such a process thus started gives to consciousness the picture or image of the object which we call a memory."[3] But that is assuming the very fact we are endeavoring to explain. If association were all that is needed to account for the image, how is it that the sensory material which it brings sometimes fuses into the percept and sometimes results in an idea?

Furthermore, the theory that the image should arise, genetically, at the very point where it is needed, is foreign to the evolutionary point of view (save to those who understand teleology). The image, to be sure, has come to be the tool of human purpose. But it was not fashioned in response to purpose. It happened. And we shall shortly have reason to believe that the situation which gives it birth is more closely akin to the day dream than to the typical problematic situation.

How then did its genesis come about? Before attempting a reply to this question, let us first make sure that we understand its meaning. We are *not* attempting to account for the presence of indirectly aroused sensory material in consciousness. Indirect arousal is not the differentia of the image.[4] From the structural point of view, we are trying to account for the presence in consciousness of sensory material which

[1] 'Psychology,' p. 215.

[2] The quotation serves my purposes so well that I could not resist temptation, even though it misrepresents the real views of the author. Professor Angell evidently does not intend the above for a detailed explanation.

[3] 'Mental Development in Child and Race,' p. 286.

[4] See almost any chapter on perception.

does *not* blend into the perceptual situation, but maintains, so to speak, a semi-independent existence of its own, and which constitutes an *object* not only to the 'artificial' introspection of the psychologist, but also the spontaneous attention of the individual. For if we believe that there ever was a conscious organism incapable of forming images, we *must* believe that *all* indirectly aroused sensory material fused with the only other form of consciousness which we know, the perceptual. What we are trying to explain is the freedom of the image.

But the facts of structure will not lead us very far in explaining other facts of structure. The significance of the image is not to be sought in the sensory clothes in which it is garbed, but in the reaction of which it is an integral part. We may hope, therefore, to shed light on our problem by studying the type of reaction peculiar to the image. Let me say at once that I share the view of those who hold that the ideational response is an indirect, nascent motor response leading, typically, to a delayed overt response. The indirect reaction is the means, the delayed reaction the end. If that view be true we may hope to solve our problem by studying the indirect reactions of the growing, developing child. And our problem is that of bringing an indirect reaction to the focus of consciousness.

Suppose now that the six-months-old infant is hungry and that the food object is not 'present to sense.' In the milder initial stages it is conceivable that the tendency will discharge into more or less overt feeding movements. With or without the aid of centrally aroused sensations that form of behavior may serve to satisfy the hunger for the time being. The satisfaction, however, will be transient, and that mode of behavior is incapable of developing into an efficient method of control. The nascent movement is, in the first instance, a direct action. It is not likely to develop into indirect behavior. Very soon the infant will express its dissatisfaction with the present status of the universe by means of entirely overt vocalization, a mode of behavior far more likely to be attended by success, but not a promising beginning for the development of thought.

Again, let us take the development of the direct movement of reaching for an object, which proves to be out of reach, into the indirect pointing gesture. That is, the child learns that reaching for an object in the presence of others is a means of having its desire satisfied. Such a response is indirect, objectively, from the physical, and direct from the social point of view. That is, the gesture does not directly effect a change in the physical object, the doll, to which it is directed, nor does the child intend that it should. It does affect directly the individual to whom it is addressed. Does this type of indirect response lend itself to the production of free imagery? I think not. I need not argue, I think, that in the presence of the perceptual object any relevant sensory material which association may bring will fuse into the percept. On the other hand, I see no reason why nascent gestures of this character should not occur in the absence of the object and constitute an integral part of the psychophysical need of that object. Those whose ideal of perfection is the logical circle may argue that the child *cannot* react to an absent object until after he has the power of creating imagery to represent that object. But whatever may follow from the logic of definition, it is a fact that much of the efficient thinking of people constituted as I am takes place in just that way. The gesture *means* the object. It does not re-present it.

Now, confining ourselves for the moment to adult consciousness, is not the nascent pointing gesture then the free image which we seek? It is capable of becoming an independent object to trained introspection. To be sure, it is not discriminated as a movement or as an image in the direct (non-introspective) situation in which it occurs. But then that is true of most of the sensory stuff usually classed as imagery. At best the degree to which such stuff is attended to, apart from its meaning, depends very largely on the training of the individual and on the temporary direction of attention. Again, I might object to classing the nascent gesture as an image on the ground that we have here kinæsthetic *sensation* rather than imagery. But such a distinction would depend upon an unproven theory. It is by no means

certain that imagery of all kinds is not due to the indirect arousal of the sense-organ. It is a matter of purely verbal definition whether we call a nascent movement an image, and it is not for reasons such as these that I am interested in the *free* image. So, as a matter of definition, and in order to make my point, I shall define the free image as the 'copy' image which not only means the object but which also resembles it.

Now it is obvious that the nascent pointing gesture does not resemble the object which it means and indicates. It may be urged, however, that it is associated with other, *e. g.*, visual imagery which can and does resemble the object. In adult consciousness such is the case with individuals of the visual type. I must admit this for the case of the adult, but I would contend that it is exceedingly improbable that the first copy image of the child arises in this way. For the nascent gesture, as such, is not at the focus of attention. Therefore imagery associated with it would not find a substantive nucleus upon which to crystallize into an ideal object. It is not the shape or color of the doll which the child desires. It wants to play. Accordingly visual imagery, if it comes at all, will find its place in the dim fringe of consciousness and will fuse into the perceptual situation of the moment.

Let us now consider another type of indirect reaction, speech. Overt speech, which most of us believe to be gestural in its genesis, is like all gesture indirect physically and direct socially. The fact that it carries meaning is quite analogous to the fact that the pointing gesture carries meaning. It differs, for our purposes, from other gestures in that it is an activity which, as a matter of fact, is carried on for its own sake. The child, an hour at a time, will prattle away contentedly, and it is safe to assume that his attention is on the activity. If now this activity is inhibited, say by direct command of authority, the conditions are just ripe, I think, for the genesis of free imagery. For the activity, being pleasurable, will tend to be continued for its own sake, will tend to express itself nascently, and will tend to reinstate the sensory material with which it has been associated, in this

case auditory. The association of this auditory imagery with the gesture is more compelling, more compact, than the sensory material brought by the pointing gesture on account of the practical immediacy of the association. That is, it is separated from the gesture with which it is associated by the briefest interval of time. Furthermore, and this is of greater importance, it serves to continue the ongoing pleasurable activity of the moment. It is successful. It is efficient. It *re*-presents the object because it almost reproduces it.

It will occupy the focus of attention because it is almost a replica not only of the object, but also of the activity which *tends* to occupy it. Indeed it really is a matter of indifference whether auditory imagery accompanies the nascent gesture provided it means to the child what the overt gesture meant. Auditory imagery may help toward this end, but it does not seem to be necessary. The problem of the genesis of the free image is really that of getting an act which is indirect *physically and socially* to the focus of consciousness. My entire point is that it is not likely to get there on account of its future usefulness as a means, but is exceedingly likely to do so if it is worth while for its own sake in the present. The free image at the moment of its genesis is a direct, though nascent act. It becomes indirect later as it becomes a means and is built into our hierarchy of habits.

From the time that the image has gained its freedom and is able to occupy the focus of consciousness as an object, its development scarcely needs sketching. Given a substantive nucleus, associated imagery will fuse into it and will enrich the direct awareness of the meaning and the sensory qualities of our ideas. It may come to pass that visual imagery associated with the nascent vocal gesture, probably along with eye movements, will come to displace it entirely. And, to the extent that our ideas become habitual means rather than ends, their richness will fade and vanish and become schematic.

It is, I think, significant to note that from the very moment of its birth the image bears the stamp of individuality and freedom. Not only does it enable the child to continue a satisfying activity, but it frees it to some extent from the

irksome bonds of external authority. The child who has learned the use and control of imagery has reached the point, for the first time in his life, where he can call his soul his own. For the nascent vocal gesture is indirect physically and *socially*.

Summary

Summing up, our argument is as follows: On the structural side, the peculiarity of the image is that it occupies—or can occupy—the focus of consciousness as an object which, somehow, belongs to a realm distinct from the perceptual. On the functional side we find that the image is the sensory aspect of a nascent activity which, as purposive activity, is indirect with reference to this perceptual realm. In the formation of habits we see that such indirect reactions are not attended to after the habit is well formed. But our habits of thought are never quite so automatic that this can take place. Accordingly we *might* classify all non-habitual purposive activity as thought. But such a classification neglects the structural differentia, the non-perceptual texture, of the idea. The suggestion of the present article is that this structural difference is correlated with the fact that thought is indirect socially, and is a development of the direct social gesture. This suggestion gains additional weight (to my mind) from the fact that, without slighting the old distinction between the objective and the subjective, it brings it nearer to common sense. The 'objective' is open to the observation of all; the subjective is experienced by the individual alone. The genesis of thought sketches the coming to consciousness of this distinction. Accordingly it will come to consciousness at a time when privacy of conscious activity *in the presence of others* is of value to the individual.

In conclusion I wish to say that it is not the contention of this paper that the mode of genesis so briefly suggested is the only one possible. I have not aimed to lay down 'the necessary conditions' for the birth of imagery. I do hope that the suggestion will be found plausible and intelligible.

After the above had been written[1] the attention of the

[1] The summary was added later.

writer was called to Miss Washburn's treatment of the 'conditions favoring the development of memory ideas.'[1] Although the similarity of Professor Washburn's account to mine is slight, it may be advisable to discuss it briefly. Professor Washburn does not confine herself to copy images as I do, but deals with ideas in general. Nevertheless she feels that attention to the movement is a necessary condition of its becoming an idea. "Thirdly, one of the conditions of the anticipation of a movement appears to be attention to it when it is originally performed. In order to remember a movement, we must have paid attention to the sensations which its performance occasions."[2] Now whatever may be true of the memory of movements as such, Miss Washburn's statement does not appear to hold of the movements which play an important part in effective thinking. So far as I can see, the eye movements which serve such a function in my own thinking need never have been at the focus of attention until my introspection discovers them. Thus if a certain book is usually to the right of my desk, my attention in actually looking for it will be on the book and on other external objects. The reaction, however, may recur in other surroundings and will *mean* the book. Are such nascent movements ideas? It was for the purpose of avoiding the fruitless discussion of this issue that I confined myself to the copy image. Again, Miss Washburn seems to think that she has solved her problem if she shows that attention has been on the movement when it was originally performed. But it is necessary also to show that attention will be on the movement—I should prefer to say 'on the activity'—as it develops into an idea. That this condition is fulfilled in the case of nascent speech is too obvious for elaboration. Miss Washburn does not mention any specific movements, though her account suggests the wiggling of the toes, the sucking of the thumb, and other infantile behavior.[3] It is far from obvious how movements such as these can develop into effective mental tools.

[1] 'Animal Mind,' second ed., pp. 302–307.

[2] *Op. cit.*, p. 305.

[3] *Op. cit.*, p. 306.

THE HETEROCHROMATIC DIFFERENTIAL THRESHOLD FOR BRIGHTNESS: I. EXPERIMENTAL.[1]

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I. The Problem.

The purpose of the experiments and measurements to be described in this article was primarily to obtain a quantitative estimate of the influence of hue difference upon the threshold for discrimination between differing brightness values. This problem is of interest from a number of points of view. In the first place, it is immediately relevant to the question of the uncertainty of 'equality of brightness' judgments in heterochromatic photometry by 'direct comparison.' Secondly, it has an important bearing upon the problem of the utility and significance of the 'just noticeable difference' as a unit for the measurement of luminosity, and other subjective sensory quantities. Thirdly, the solution of the problem might be expected to throw some light upon the relations of the compared hues themselves.

Fundamental Terms and Definitions.—There are a number of separate factors in the problem which should be clearly distinguished at the outset. In all psycho-physical work, we are forced to deal with two fundamental, parallel quantities or dimensions, those of the *sensation* and of the *stimulus*, respectively. In the present paper, we shall employ the term *luminosity* to stand for the intensity factor of the *sensation;* it will be used as a synonym of 'apparent brightness.' Strictly speaking, the intensity term of the *stimulus* is the radiometric analogue of retinal illumination, *i. e.*, watts per unit retinal area.[2] Expressed in terms of objective *light*,

[1] From the Nela Research Laboratory of the General Electric Co., Cleveland, Ohio.

[2] See my paper, 'On the Measurement of Visual Stimulation Intensities,' J. of Exp. Psychol., 1917, 2, 1–34.

this intensity is measured in lumens per square meter, or in meter-candles. However, the retinal illumination is approximately proportional, with a constant pupillary opening, to the *photometric brightness* of the external stimulus surface, and consequently the stimulus intensity can be expressed very conveniently by means of a unit which the writer has called the *photon*,[1] which represents the state of retinal stimulation accompanying a stimulus surface brightness of one candle per square meter and an effective pupil of one square millimeter. This unit may be said to measure the *physiological intensity* of the stimulus. It is an approximate unit of retinal illumination.

There are at least three possible interpretations of the general term, 'differential threshold for brightness,' which it is important to keep separate in our discussion. They are: (1) the *sensory* (or luminosity) threshold, which is equal, by definition, in the Fechnerian scheme of mental measurement, to one luminosity unit or step; (2) the absolute *stimulus* (or light) threshold, which is the arithmetic difference between the two stimulus values which underlie any single luminosity step; and (3) the *relative* stimulus (or light) threshold, which is the ratio of (2) to the higher of the two absolute light values entering into the comparison. (2) is sometimes called the 'sensibility' to differences in light intensity, and (3) the 'Fechner fraction.' Nutting has recently named the reciprocal of this fraction 'the discrimination factor.'[2]

Both the absolute and the relative stimulus thresholds can be considered in reference to (1) the retinal illumination, or (2) the photometric brightness of the external stimulus surfaces. They can be expressed in radiometric as well as in photometric units, and can be derived from measurements made in either of these units. However, with a specified and constant pupillary opening, the proportionality existing between retinal illumination and photometric bright-

[1] *Loc. cit.*, pp. 27–33.

[2] Nutting, P. G., 'Effects of Brightness and Contrast in Vision,' *Transactions of the Illuminating Engineering Society*, 1916, 11, 945.

ness permits us to treat these latter measures as if they were identical, if our object is to study only the *relative* stimulus threshold. For any one wave-length of radiation, the photometric and radiometric values are also proportional, and their ratios for different wave-lengths are given by the visibility curve. In the present discussion our principal concern is with influence of color difference upon the *relative* threshold; absolute intensities will be expressed in photons.

Formulation of the Problem.—The nature of our specific problem may be described somewhat more in detail as follows: Suppose that C and S are two stimulus fields, filled uniformly with light. Let the intensity of the stimulus in C be h and in S be i, and let us represent the *color* of C by λ_1, and of S by λ_2, without assuming that λ_1 and λ_2 stand for single wave-lengths.

If Δi is the absolute stimulus threshold, and $i - h = \Delta i$, the relative stimulus threshold must be

$$t = \frac{i - h}{i} = \frac{\Delta i}{i}, \tag{1}$$

assuming $i > h$.

It is a well-established fact that the value of t, although remarkably constant, is nevertheless to some extent a function of practically all of the variables involved in the visual process. Its dependence upon absolute intensity and upon wave-length has been studied very thoroughly by König,[1] and more recently by Nutting.[2] The value of the fraction is influenced, also, by the size of the comparison fields, and by the steepness of the brightness gradient at their junction, if they adjoin, or by their degree of separation, if they are not in juxtaposition. The small differences which exist between the values of the threshold for lights of various wave-lengths, when both of the comparison fields are of the same color, indicate that *color difference* between the two fields must be treated as a factor more or less independent of absolute

[1] König, A., and Brodhun, E., 'Experimentelle Untersuchungen über die psychophysische Fundamentalformel in Bezug auf den Gesichtssinn,' *König's Gesammelte Abhandlungen zur Physiologischen Optik*, 1903, 116–140.

[2] *Loc. cit.*, 944–946.

color, since in heterochromatic photometry by direct comparison a very marked increase in the threshold is encountered.

If the color difference between the fields, C and S, be represented by $\lambda_2 - \lambda_1$, our problem may be stated as that of determining the general form of the function, $f(\quad)$, in

$$t = f(\lambda_2 - \lambda_1). \tag{a}$$

Color and color difference can be expressed in terms of position on a subjective hue scale, as well as in terms of wavelength, so that it will be possible to rewrite (*a*) in psychological units, a transformation which may prove of theoretical, if not of practical interest.

II. The Literature of the Subject

So far as the writer has been able to determine, no systematic studies have previously been made upon the problem formulated above. The very small amount of data in the literature, which bear upon it, consist in the mean variations of measurements in heterochromatic photometry, usually involving a comparison between a 'white' standard and a color. However, in many of the researches relating to heterochromatic photometry, quantitative estimates of the precision of the measurements appear not to have been made. The mean variations of König's classical visibility measurements are given as of the order of magnitude of 5 per cent.[1] Most of the later work on visibility, having been done by the method of flicker, throws little light upon our problem. Helmoltz[2] found that with his color-mixing apparatus, the mean variation of 25 observations, was 3.0 per cent. for the comparison of red with red, 3.3 per cent for the comparison of blue with blue, and 5.8 per cent. for the comparison of red with blue.

Ferree and Rand[3] state the mean variation of photo-

[1] König, A., 'Ueber den Helligkeitswerth der Spectralfarben bei verschiedener absoluter Intensität,' *König's Gesammelte Abhandlungen zur Physiologischen Optik*, 1903, p. 184.

[2] Helmholtz, H. von, 'Handbuch der physiologischen Optik,' IIte Auflage, 1896, p. 430.

[3] Ferree, C. E., and Rand, G., 'A New Method of Heterochromatic Photometry,' J. of Exp. Psychol., 1916, 1, p. 9.

metric equations between a (tungsten) 'white' standard and a red of 768–650 $\mu\mu$ to be 2.5 per cent. and 'the smallest amount of change which can be detected' is 4.8 per cent. on the average; for a blue-green of 465–520 $\mu\mu$ (compared with the white) the corresponding values are 4.5 per cent. and 8.0 per cent. In other work,[1] using a 0.9 degree field, the same workers found the least perceptible change in the brightness of the white in comparison with the above specified red to be 6.6 per cent. for one observer, and 7.0 per cent. for another; with the green, 6.6 per cent. for the first observer and 8.4 per cent. for the second.

Recently, very exhaustive tests have been made by Crittenden and Richtmyer[2] on the precision of photometric comparisons of lights showing relatively small color differences, representative of various modern types of illuminants. The mean of the residuals for such comparisons between the light of a 4 w. p. c. carbon lamps and a vacuum tungsten at about 1.2 w. p. c.—or equivalent combinations of colored glass filters—was found to be 1.9 per cent. for 114 observers. The observers could be divided into three classes, 31 showing a mean variation of 1.2 per cent; 58, one of 1.5 per cent; and 25, one of 3.5 per cent. with a 2-degree field.

The most interesting data available seem to be those presented graphically by Ives, in his well-known monograph on the flicker photometer,[3] from which the figures in Table I. are drawn. They represent the average of the 'mean errors' (m.v.) made by five observers in the equation by direct comparison, of spectral lights of the given wave-length with the light from a 4.85 w.p.m.s.c. carbon lamp, reflected from a magnesium oxide surface. The field size was 4.58 degrees (diameter), and the two intensities employed were equivalent

[1] Ferree, C. E., and Rand, G., 'A Preliminary Study of the Deficiencies of the Method of Flicker for the Photometry of Lights of Different Colors,' Psychol. Rev., 1915, 22, 139.

[2] Crittenden, E. C., and Richtmyer, F. K., 'An "Average Eye" for Heterochromatic Photometry, and a Comparison of a Flicker and an Equality-of-Brightness Photometer,' *Transactions of the Illuminating Engineering Society*, 1916, 11, 331–356, esp. 344.

[3] Ives, H. E., 'Studies in the Photometry of Lights of Different Colors,' I., *Phil. Mag.*, 1912 (6), 24, Plate III., Figs. 2 and 3.

to 76.0 and 30.4 photons, respectively. An examination of the table shows that there is a tendency for the value of the variation to be larger at the ends of the spectrum than in the middle, a fact which may be attributed either to the

TABLE I

PERCENTAGE MEAN VARIATIONS OF PHOTOMETRIC EQUATIONS BY DIRECT COMPARISON BETWEEN A 4.85 W.P.C. CARBON LAMP STANDARD WHITE AND SPECTRAL COLORS (FROM IVES)

Wave-Length	M. V. at Intensity of 76.0 Photons	M. V. at Intensity of 30.4 Photons
653	8.3	5.6
643	6.9	5.3
632	6.3	4.3
622	6.5	3.3
612	4.5	3.9
594	3.7	3.1
574	5.2	2.2
555	4.7	4.1
545	7.9	4.9
535	3.1	3.6
526	4.7	6.0
517	7.0	6.7
Averages	5.74	4.43

relatively low inherent saturation of the mid spectral colors, or to their closer approximation in hue to the unsaturated color of the standard, or to both causes. The average mean variation, for all colors, at 76.0 photons is 5.74 per cent. and at 30.4 photons is 4.43 per cent.

III. APPARATUS AND EXPERIMENTAL METHOD

Description of Apparatus.—The arrangement of the apparatus which was used in the present work is shown schematically in Fig. 1. The principal element is a Lummer-Brodhun spectrophotometer, which has been provided with an additional collimator system, C_3. The light from this system is reflected by an adjustable and removable mirror, M_2, so that it exactly replaces the light naturally proceeding from C_2. B is a regular Lummer-Brodhun cube, with the usual contrast pattern, but the contrast strips have been removed. The rotation of M_2 about a vertical axis by means

of a micrometer screw determines the portion of the spectrum which passes through the telescope slit, S_4. The light from a straight filament lamp, L_4, can be thrown upon M_2 by means of the lens, N_4, in such a way that an image of the filament is formed on the scale, *Sc*; this permits the calibration of the positions of the mirror with respect to the wave-length passing through S_4, from C_3.

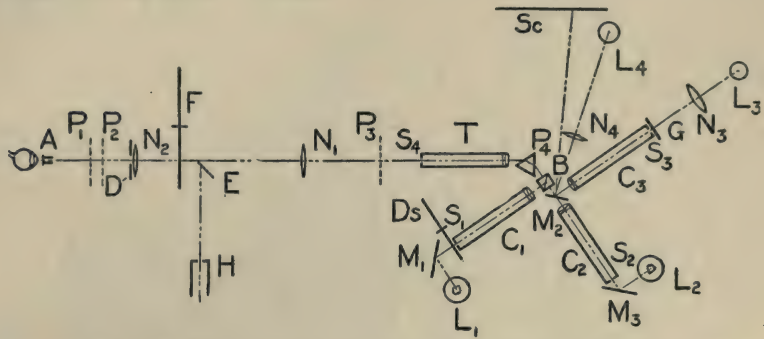


FIG. 1. Schematic plan of apparatus. (See text for explanation.)

The light from the collimator, C_1, is reflected in the prism, *B*, in the usual manner, from the remaining portion of the photometric field pattern. C_2 is used only for *homochromatic* comparisons, by removal of the mirror. The slits, S_1 and S_2, are illuminated by the light from 100-watt, 110-volt Mazda C lamps, reflected from thin plate-glass mirrors, M_1 and M_2, respectively, the surfaces of which have been ground with 60′ carborundum. In front of S_1 is a Hyde variable sectored disk,[1] *Ds*, which is rotated at a high speed by an electric motor. The slit, S_3, is illuminated by the image of the small coiled filament of the Mazda C automobile headlight lamp, L_3, which is condensed by the lens, N_3, upon the finely ground diffusing glass, *G*. The candle-power of L_3 can be varied, with insensible gradations, by means of a rheostat. L_1 and L_2 are operated on a 110-volt generator circuit of constant voltage, and L_3 on storage cells.

The light from the slit, S_4, is caught by the 3.5 diopter

[1] See Hyde, E. P., 'Slit-width Corrections in Spectrophotometry and a New Form of Variable Sectored Disk,' *Astrophysical Journal*, 1912, 35, 257–267.

lens, N_1, and converged sufficiently so that the cone is completely intercepted by a second lens, N_2—of 3.25 diopters—which converges the light again, upon the artificial pupil, A. Just in front of N_2 is a diaphragm, D, which limits the size of the illuminated field, seen by the subject. F is a flicker photometer disk, used in measuring the stimulus brightnesses, and E is a white reflecting surface which can be brought into one half of the field, when desired, and illuminated by the spectral light from a Hilger monochromatic illuminator, H.

Calibration of M_2 and C_3 was accomplished by means of color-matches, with the aid of the last-mentioned arrangement. This method—because of the necessity of repeated recalibration—was the only practicable one, and for the most important regions of the spectrum is as sensitive as the direct physical method (using a helium tube) which was employed in the calibration of C_1. C_2 was set by color match in the yellow, so as to give the same range of the spectrum as did C_1 for all positions of the telescope, T. The scale on the wave-length drum of the Hilger instrument was carefully tested with the sodium line, and found to be correct.

At P_1, P_2 and P_3, small totally reflecting prisms—not shown in the drawing—were employed to change the path of the light. The first two prisms formed part of the writer's artificial pupil apparatus, with its arrangement for shifting the stimulus from one eye to the other without altering the optical point of view; and the third was necessitated by limitations of space in the disposal of the instruments. Care was taken to keep all of the glass surfaces well polished, and tests showed that the amount of scattered light which they introduced was so small as to be negligible, except in certain cases where special precautions were taken to eliminate it.

A solid and comfortable head-rest was employed, in front of A, and a square artificial pupil, 2.51 mm. on a side, was used. When the observer placed his eye before this he saw a clearly defined image of the pattern of the Lummer-Brodhun cube, B, projected on the lens, N_2, but delimited by the diaphragm, D. All of the measurements were made with the right eye,

and approximate registration of the natural pupil with respect to the artificial pupil was secured through adjustments of the head—by trial and error—so as to give a maximum and constant apparent brightness to the field.

Owing to the different angles of refraction of the lights used in the two portions of the Lummer-Brodhun field, there was a marked overlapping— to the right or left—of the colors, when these were at all widely separated in the spectrum. To eliminate this disturbing effect, the diaphragm, *D*, was cut in such a way as to exclude the parts of the field in which this overlapping occurred. The resulting field pattern is drawn, to scale, in Fig. 2, the over-all dimensions being 2.62

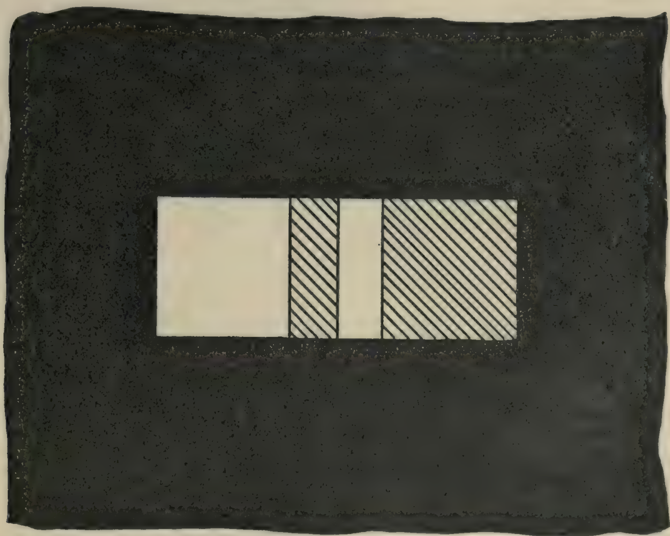


FIG. 2. The pattern of the photometric field. The shaded portion was occupied by the 'standard' color, *S*, the unshaded portion by the 'comparison' color, *C*.

by 1.02 degrees, and the width of each of the middle bands, 0.34 degrees. The general pattern remained unchanged throughout the experiment. The shaded portion (in Fig. 2) was occupied by the standard color (*S*), derived from C_1, and the unshaded part by the comparison color (*C*), derived from either C_3 (heterochromatic comparison) or C_2 (homo-

chromatic comparison). The field was viewed against a completely dark background, and while a judgment was being made, attention was directed to the central line of demarcation, so that the image of the field must have fallen entirely upon the rod-free region of the retina, which is estimated as about three degrees in diameter.[1]

The general conditions of observation were made, as closely as could be, the same as those surrounding general photometric work. The observer's eyes were protected from all direct, glaring light, but the room was diffusely illuminated by the lamps forming part of the apparatus. This diffuse light, however, was quite cut off from the visual field during an observation. Since stimulation was confined to the cones, in the experiment, the degree of scotopic adaptation of the subject's eye was a matter of secondary or of no importance.

Experimental Procedure.—Four colors, corresponding in spectral position with the so-called psychological primaries, red, yellow, green, and blue, were chosen as standards. The mean wave-lengths of the spectral ranges employed were 693, 575, 505, and 475 $\mu\mu$, respectively. These standards were compared with each other, and with nine additional colors taken at intervals of 30 $\mu\mu$ across the spectrum, starting with 430 $\mu\mu$, which was the farthest violet which could be obtained in sufficient intensity. To reduce the proportion of scattered white light, introduced by the natural defects of the spectrometer system, color filters of appropriate selective transmission were employed before the collimator slits for lights lying at the extremes of the spectrum.

The magnitudes of the wave-length ranges of the standards and comparison colors, as calculated from the slit-widths, and other dimensions of the optical systems, are given in Table II. The ranges of the comparison lights used with the blue standard differ from those for the other three standards, because of the use of a wider telescope slit, S_4, which was necessary in order to obtain a field brightness equal to that of the other standards. The range estimates given in the table are accurate to about 1 $\mu\mu$. The accuracy of control of the

[1] See Parsons, J. H., 'An Introduction to the Study of Color Vision,' 1915, 10.

'total color' of the ranges concerned, as determined by color-matching is greater than this in the regions of the spectrum where hue changes rapidly with respect to wave-length, and less in regions where the hue change is small.

TABLE II

WAVE-LENGTH RANGES, IN $\mu\mu$, OF STANDARD AND COMPARISON COLORS USED IN THE EXPERIMENTS, AS DETERMINED FROM SLIT-WIDTHS AND OTHER DATA

A. Standard Colors

Mean Wave-length	Limiting Wave-lengths	Range
693	678–708	30
575	570–580	10
505	499–511	12
475	469–481	12

B. Comparison Colors

Mean Wave-length	Limiting Wave-lengths		Range	
	Blue Standard	Other Standards	Blue Standard	Other Standards
693	676–711	682–704	35	22
670	654–686	660–680	32	20
640	626–654	631–649	28	18
610	598–622	602–618	24	16
580	569–591	573–587	22	14
575	565–585	568–582	20	14
550	541–559	544–556	18	12
520	513–527	515–525	14	10
505	499–511	500–510	12	10
490	484–496	486–494	12	8
475	470–480	471–479	10	8
460	456–464	457–463	8	6
430	427–433	428–432	6	4

The principal series of measurements was made at a constant intensity of approximately 25 photons. This corresponded with the highest brightness of the red standard obtainable with a 0.5 mm. collimator slit, and a 0.4 mm. telescope (or ocular) slit. The intensities of the remaining three standards were equated to that of the red by regulating the two slit-widths and, in the case of the yellow and green, also by placing an absorption glass before the collimator lens.

These measurements and equations were accomplished by means of a flicker photometer, using a white disk and a vacuum tungsten lamp, calibrated with respect to voltage in

candles per square meter (brightness of the illuminated disk). The statements of intensity here given are corrected for the absorption of the lens, N_2, and the prisms, P_1 and P_2 (which intervene between the flicker disk and the eye), as well as for the fractional interception of light by the disk, *Ds*, which was effective during the threshold measurements, although this disc was stationary, and out of the path of the light, when the flicker determinations were being made.

The average accuracy of reduplication of the *absolute* intensities between trials was about 9 per cent. Since a variation of even 100 per cent. has only a slight effect upon the magnitude of the thresholds, this accuracy may be considered satisfactory. It was conditioned by the method employed to establish intensities at the beginning of each series of measurements. The standard was first given its proper value by setting the slits and the rotating sector disk at the scale positions which had been determined for 25 photons. The comparison field was then matched in brightness to the standard by direct (heterochromatic) comparison, through the adjustment—by means of a rheostat—of the voltage across the lamp, L_3. The voltage required for the match was noted, and the lamp was burned at this voltage until the comparison color was changed, when the whole process had to be repeated. The accuracy of maintenance of the candle-power of L_3 during a series of measurements was better than ½ per cent. for the majority of cases.

The threshold measurements proper were made by changing the brightness of the standard itself, through alteration of the position of the rotating sector disk. The disk was controlled directly by the observer, through a system of cords and pulleys. Its scale had been carefully calibrated in terms of the fraction of total light transmitted. A telescope and small lamp were provided so that the scale could be read from the observer's position, although in about one half of the cases the readings were actually taken by an assistant.

In determining the threshold the 'method of limits' was employed. The brightness of the standard, *S*, was first increased until it was decidedly greater than that of the com-

parison field, C, and then it was gradually decreased until it was *just not noticeably brighter* than—or just equal to—C. It was then decreased further until it appeared *just noticeably darker* than C, then increased again until it was *just not noticeably darker*, and finally it was increased further until it was *just noticeably brighter* than C, scale readings being taken at each point. This cycle was repeated five times for each series, making twenty determinations in all. On the average, about six such series could be made at a single sitting, without unduly fatiguing the observer, and as a rule the comparison lights were taken in spectral order. All series of observations in which a progressive drop in successive readings occurred, were rejected and repeated owing to a suspicion of voltage changes. Two observers took part in the work, one—the writer (T.)—having had considerable experience in photometric and visual work, and the other—Mr. Robert Lucas (L.)—being entirely without such experience, but a very careful worker. Two complete sets of measurements were made by the writer and one complete set by Mr. Lucas. Both observers possess normal trichromatic vision.

An extra set of measurements, against a green standard only, was made by the writer at an intensity of 240 photons, in order to study the influence upon the threshold values of small changes in intensity.

No general systematic attempt was made to evaluate the 'space errors' involved in the comparison, since the position of the standard in the field could not readily be changed. However, automatic interchange of the standard colors among themselves naturally occurred on account of their duplication in certain of the comparison colors. Owing to the method employed in the calculation, the term 'space error' cannot be given its usual meaning of an increase or decrease in the apparent value of one of the compared stimuli. It must rather be regarded as an influence exerted upon the magnitude of the threshold itself, according as one color or the other occupies a given portion of the test field, and is made the variable quantity.

IV. The Calculation of the Threshold Values

The values of the thresholds were computed from the scale readings by the following method.

First, the individual values of the scale readings for each of the four *points:* (*a*) just noticeably brighter, (*b*) just not noticeably brighter, (*c*) just noticeably darker, and (*d*) just not noticeably darker, were averaged separately for each different set of conditions. Since the function connecting scale readings with 'transmission' or relative brightness, was of approximately logarithmic form,

$$(2) \qquad r = k \log b + k',$$

the 'transmissions' looked up in the calibration table of the disk, corresponding with the averages of the scale-readings (r) would be the geometrical means of the individual transmissions corresponding with the individual scale-readings. Although the difference between the geometrical and arithmetical means in this computation is small, the geometrical mean of the brightnesses is theoretically to be preferred.[1] The geometric means of the averages for (*a*) and (*b*), and for (*c*) and (*d*), respectively, were found by the same method.

Let us designate the first of these values—the average of the brightnesses for 'just noticeably brighter' and 'just not noticeably brighter'—by i, and the second—the average of the brightnesses for 'just noticeably darker' and 'just not noticeably darker'—by j. By derivation: $i = \sqrt{ab}$, and $j = \sqrt{cd}$. Then, if the unknown brightness of the constant comparison field, C, is taken as h, we have, from Weber's law:

$$(3) \qquad h/j = i/h,$$

since $h - i$ and $j - h$ are both threshold steps. Hence

$$(4) \qquad h = \sqrt{ij}$$

and two equivalent expressions for the relative stimulus threshold, or Fechner fraction, may be written, viz.,

[1] This statement rests on the assumption that an (arithmetic) average based upon a response or sensation scale is to be preferred to one based upon a stimulus scale, the relation between the two scales being logarithmic. An arithmetic average on the response scale obviously corresponds with a geometric one on the stimulus scale.

$$(5) \qquad t = \frac{\sqrt{ij} - j}{\sqrt{ij}} = \frac{i - \sqrt{ij}}{i}.$$

These formulæ follow the convention, recommended by Nutting, of employing the higher of two liminally different brightness as the denominator of the Fechner fraction.

The values of t as computed by means of the last term of (5) for each of the fifty-two different conditions of comparison, at an intensity of 25 photons, and for both subjects, are given in Table III. Each value for subject L. represents at least twenty independent observations, and for subject *T.* from forty to one hundred. Additional measurements were made in certain cases to increase the precision of seemingly aberrant values. The computation of $\sqrt{ij}$ was done on the slide-rule rather than in terms of scale readings, because of the appreciable deviation of the calibration curve from logarithmic form over the large range of scale-readings involved between i and j.

Under v in Table III. are given the average mean variations of the individual brightness values of the four 'points,' (*a*), (*b*), (*c*), and (*d*), expressed as fractions of these values. The magnitudes of v, for each of the fifty-two situations, were computed in the following manner.

First, the mean variations of the scale-readings from their mean were found for each of the points. Owing to the approximately logarithmic form of the calibration curve, a variation of one linear unit of scale-reading could be taken to represent a constant fractional variation in 'transmission' or brightness, viz., 7.2 per cent. per millimeter ($1/k$ of equation 2). Hence, by multiplying the mean variations of the scale-readings by this constant, the *fractional mean variation* of the brightnesses could be found. This fraction was first calculated for each of the four points, and then the arithmetic average, v, of the four fractions was determined for each set of conditions.

The results given in Table III. are shown graphically in Figs. 3 to 6, inclusive. In Table IV., the values of t and v are given for the series of measurements made by subject *T* at 240 photons, with a green standard.

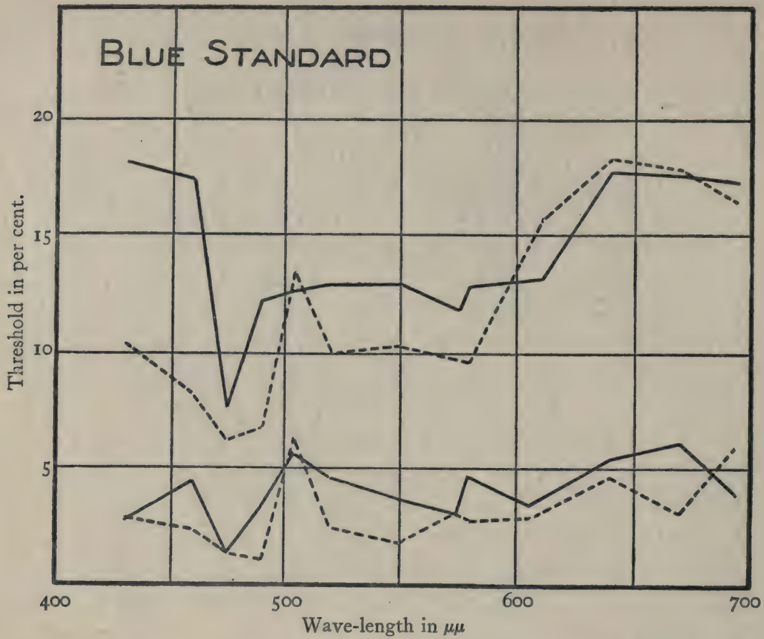


FIG. 3. Threshold and variation values for the blue standard. The values for subject T. are given by the full line, those for subject L. by the broken line.

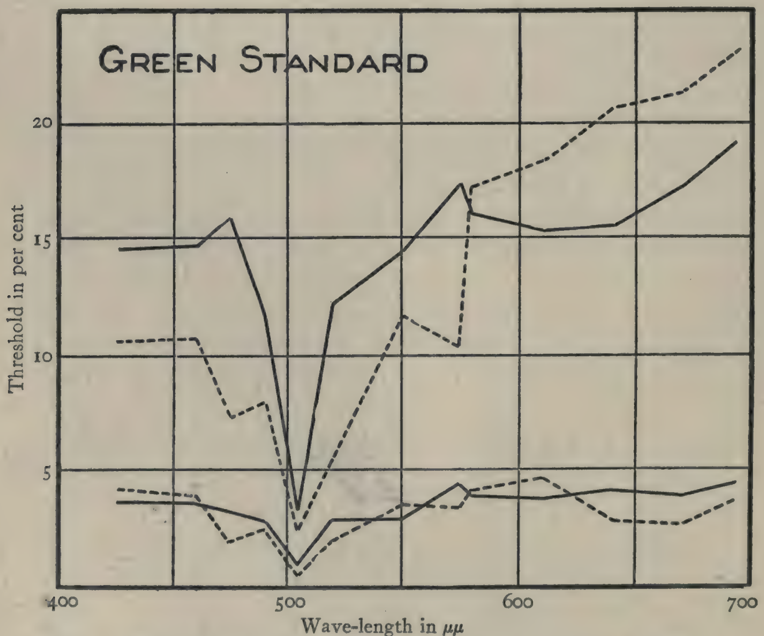


FIG. 4. Threshold and variation values for the green standard. See Fig. 3.

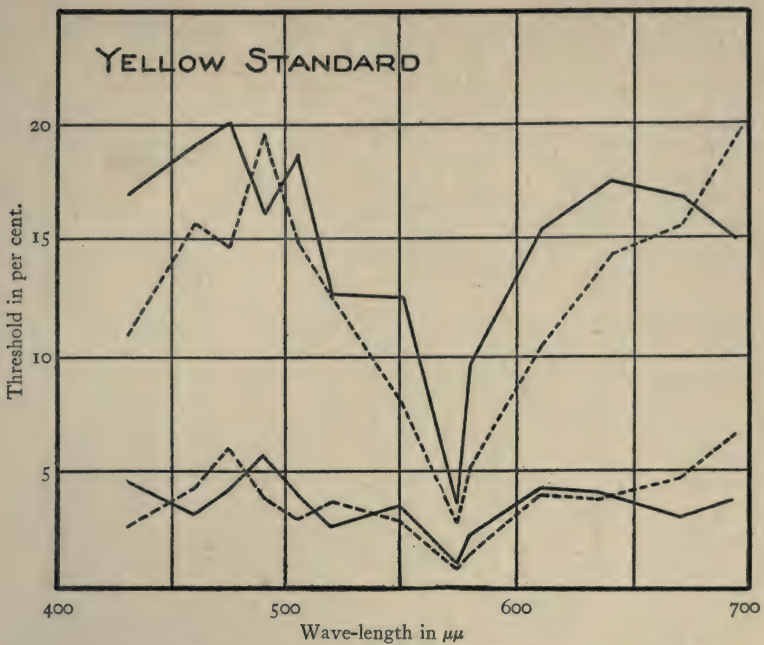


FIG. 5. Threshold and variation values for the yellow standard. See Fig. 3.

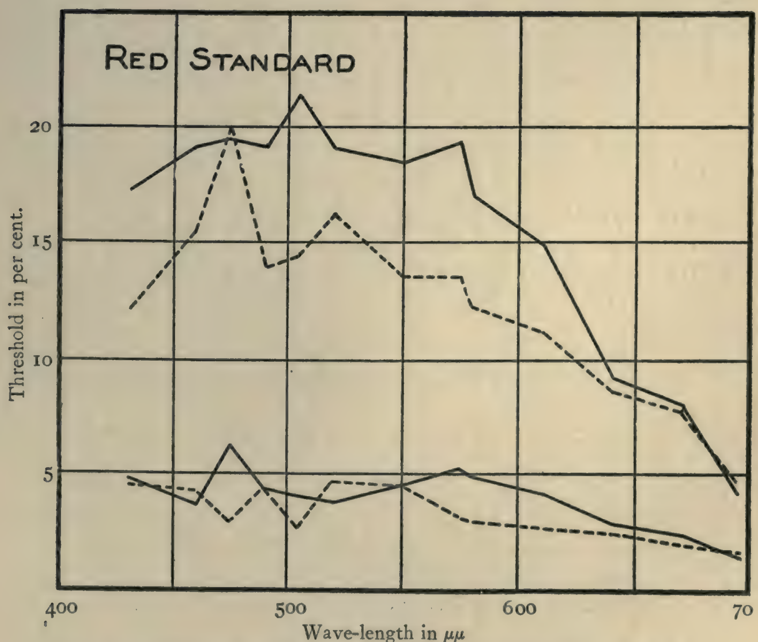


FIG. 6. Threshold and variation values for the red standard. See Fig. 3.

Table III

Values of the Heterochromatic Relative Threshold, *t*, for Brightness Discrimination, and the Average Fractional Mean Variation, *v*, of the Determining Points of the Threshold, for Two Subjects, T. and L., Four Standard Colors, and Thirteen Comparison Colors, at 25 Photons

Comparison Color, $\mu\mu$	Standard Color, $\mu\mu$							
	Blue, 475		Green, 505		Yellow, 575		Red, 693	
	t	*v*	*t*	*v*	*t*	*v*	*t*	*v*
430, (T.)	.181	.029	.145	.036	.169	.046	.174	.048
(L.)	.104	.027	.107	.042	.109	.026	.122	.045
460, (T.)	.174	.045	.147	.035	.191	.032	.191	.036
(L.)	.081	.023	.149	.038	.156	.043	.154	.042
475, (T.)	.075	.013	.159	.032	.201	.042	.195	.062
(L.)	.062	.013	.072	.019	.147	.060	.199	.028
490, (T.)	.121	.033	.116	.028	.161	.057	.191	.044
(L.)	.067	.010	.079	.024	.196	.039	.139	.044
505, (T.)	.126	.056	.0367	.0095	.185	.041	.214	.040
(L.)	.134	.062	.0237	.0035	.148	.030	.144	.026
520, (T.)	.129	.046	.122	.029	.126	.026	.190	.037
(L.)	.100	.025	.055	.019	.125	.037	.161	.046
505, (T.)	.129	.036	.144	.029	.125	.035	.185	.045
(L.)	.103	.018	.117	.035	.080	.029	.135	.045
575, (T.)	.118	.030	.173	.044	.0369	.010	.194	.052
(L.)	.141	.030	.103	.033	.0275	.0075	.135	.032
580, (T.)	.128	.046	.160	.039	.097	.023	.170	.049
(L.)	.095	.027	.172	.040	.052	.013	.122	.030
610, (T.)	.131	.034	.153	.038	.155	.043	.149	.041
(L.)	.157	.028	.184	.046	.104	.040	.111	.027
640, (T.)	.178	.053	.155	.041	.175	.041	.092	.028
(L.)	.183	.046	.206	.028	.144	.039	.085	.024
670, (T.)	.176	.060	.172	.039	.169	.031	.080	.024
(L.)	.178	.030	.213	.026	.156	.047	.076	.020
693, (T.)	.173	.039	.192	.044	.150	.038	.042	.014
(L.)	.164	.059	.231	.037	.199	.066	.047	.016

Table IV

Values of the Heterochromatic Relative Threshold, *t*, for Brightness Discrimination, and the Average Fractional Mean Variation, *v*, of the Determining Points of the Threshold, for One Subject, T., a Green Standard, 505 $\mu\mu$, and Twelve Comparison Colors, at 240 Photons

Comparison Color, $\mu\mu$	*t*	*v*
460	.175	.051
475	.108	.022
490	.165	.039
505	.0226	.0092
520	.140	.027
550	.152	.045
575	.082	.038
580	.147	.042
610	.213	.048
640	.183	.040
670	.170	.034
693	.187	.053

V. Discussion of Results: *A*. The Relation between Color Difference and the Relative Stimulus Threshold

In discussing the relations existing between the values tabulated above, we shall consider two general problems: (1) the nature of the dependence of the relative stimulus threshold upon color difference, from the point of view, first, of the threshold itself and, second, of the system of colors, and (2) the correlation existing between the threshold and its variation measure. These problems will be discussed (*a*) in their bearing upon practical photometry and (*b*) in relation to the theory of threshold and of color difference.

Comparison with König's Data.—The four *homochromatic* threshold ratios for subject T. are : *R*, .042; *Y* .0369; *G*, .0367; and *B*, .075. For subject L. the corresponding values are: .047, .0275, .0237, and .062. König, at approximately the same intensity (50 units on his scale) found: *R*, .0376; *Y*, .0320; *G*, .0252; and *B*, .0250, for nearly the same selection of wave-lengths.[1] The average value of the homochromatic threshold for subject T. is .0453; for subject L., .0403; for König, .0300.

The larger value of the homochromatic threshold obtained in the present measurements, as compared with König's results, may be attributed to a difference in any of the various factors which influence the magnitude of the threshold. It is probably not due so much to the personal equation as to the fact that König's test-field was much larger than the one employed by the writer, the over-all dimensions of the former being 6 × 4⅓ degrees. König's field, also, was divided into two, instead of into four parts. As shown in the table of König's values given by Nutting, an intensity of 25 photons is in the region of relatively rapid change of the threshold ratio as a function of intensity, and some uncertainty exists as to the exact value of König's stimulation units.

The homochromatic threshold for subject T., with the

[1] See Nutting's recalculation of König's data in 'The Luminous Equivalent of Radiation,' *Scientific Papers of the Bureau of Standards*, No. 103, 1908, 286.

green standard, at 240 photons, was 0.226, which is considerably lower than the average for 25 photons.

König's results, for intensities in the neighborhood of 25 photons, show a decrease in the value of the homochromatic threshold in passing from the red to the blue end of the spectrum, whereas in our data the threshold in the blue is, for both subjects, approximately twice the average value of the other homochromatic thresholds. The decrease shown in König's results is probably due to the increased ratio of rod response to cone response in the blue as compared with the red, as his field was sufficiently large to stimulate an appreciable number of rods. In the present writer's experiments, however, the stimulation was practically limited to the rod-free region of the retina.

The 'Heterochromatic Comparison Factor' as a Function of Color Difference.—In order to abstract the influence of color difference upon the threshold from that of other variables each threshold value can be divided by the corresponding homochromatic value, *i. e.*, all of the thresholds can be expressed in terms of the homochromatic threshold as a unit. This expression, or ratio, may be called the *heterochromatic comparison factor*. Such a reduction of course assumes that the influences exerted by the other factors are themselves independent of color difference. Although this assumption is probably not completely accurate, it may be regarded as a useful approximation to the truth, since within certain widely separated limits the influence of the other factors is small compared with that of color difference.

The values of the heterochromatic factor as calculated for all of the thresholds determined at 25 photons intensity, are given in Table V.

In considering the relation between the heterochromatic comparison factor and color difference, it is desirable to express the latter not in terms of wave-length intervals, but in terms of the number of 'just noticeable differences in hue' between any two compared colors. In certain regions of the spectrum, principally in the red, a large wave-length difference may correspond to almost no color difference at all. Fortunately,

Table V

Values of the Heterochromatic Comparison Factor, *f*, for Two Subjects, T. and L., Four Standard Colors, and Thirteen Comparison Colors, at 25 Photons

Comparison Color, $\mu\mu$	Standard Color, $\mu\mu$			
	Blue, 475	Green, 505	Yellow, 575	Red, 693
430, (T.)	2.42	3.97	4.54	4.20
(L.)	1.68	4.52	3.96	2.58
460, (T.)	2.32	3.99	4.97	4.60
(L.)	1.31	6.28	5.66	3.26
475, (T.)	1.00	4.33	5.45	4.68
(L.)	1.00	3.04	5.36	4.21
490, (T.)	1.62	3.17	4.37	4.58
(L.)	1.08	3.32	7.12	2.94
505, (T.)	1.68	1.00	5.00	5.15
(L.)	2.17	1.00	5.38	3.04
520, (T.)	1.72	3.32	3.43	4.58
(L.)	1.62	2.31	4.55	3.41
550, (T.)	1.72	3.91	3.40	4.44
(L.)	1.67	4.93	2.91	2.85
575, (T.)	1.58	4.71	1.00	4.65
(L.)	2.27	4.54	1.00	2.85
580, (T.)	1.71	4.35	2.64	4.09
(L.)	1.55	7.24	1.91	2.57
610, (T.)	1.75	4.16	4.20	3.58
(L)	2.54	7.75	3.76	2.33
640, (T.)	2.38	4.23	4.75	2.22
(L.)	2.97	8.69	5.23	1.79
670, (T.)	2.35	4.69	4.57	1.93
(L.)	2.88	9.00	5.67	1.59
693, (T.)	2.31	5.22	4.07	1.00
(L.)	2.66	9.73	7.22	1.00

Table VI

Positions of Colors, Used in the Heterochromatic Comparisons, on Nutting's Hue Scale, and Intervals, in Terms of Units of the Scale, between Comparison Colors and Standards

Mid Wave-Length, $\mu\mu$	Hue Scale Position	Hue Scale Interval from Standard,			
		475	505	575	693
430	0.02	−0.22	−0.39	−0.66	−0.98
460	.17	.07	.24	.51	.83
475	.24	.00	.17	.44	.76
490	.34	+0.10	.07	.34	.66
505	.41	.17	.00	.27	.59
520	.47	.23	+0.06	.21	.53
550	.56	.32	.15	.12	.44
575	.68	.44	.27	.00	.32
580	.71	.47	.30	+0.03	.29
610	.84	.60	.43	.16	.16
640	.91	.67	.50	.23	.09
670	.98	.74	.57	.30	.02
693	1.00	.76	.59	.32	.00

Nutting and Jones have worked out very carefully a subjective hue scale of the required sort.[1] The approximate positions, upon Nutting's scale, of the thirteen mid wavelengths employed in our experiments are given in Table VI., and the mean heterochromatic factors, for both subjects, are plotted in Fig. 7, as functions of the distance on the hue scale between the standard and the given comparison colors.

An examination of Fig. 7 brings out a number of interesting points.

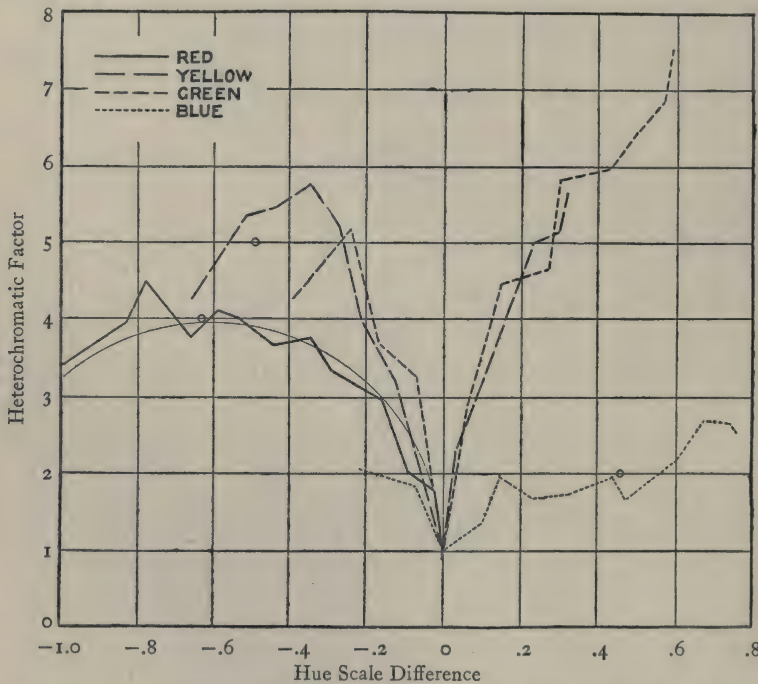


FIG. 7. The heterochromatic comparison factor, as a function of the hue scale interval between the compared colors. The curves represent the average of the results for both subjects.

The relatively small values of the heterochromatic factors obtained with the blue standard are referable, for both subjects, to low values of the heterochromatic thresholds, but

[1] Nutting, P. G., 'The Retinal Sensibilities Related to Illuminating Engineering,' *Transactions of the Illuminating Engineering Society*, 1916, 11, 15.

also and primarily to the very high values of the homochromatic threshold. The cause of this difference between the blue and the other curves is obscure, and the discrepancy is so great that the former set of values must be suspected of being affected by factors which are irrelevant to our present discussion. The difference can scarcely be attributed to an experimental error, depending upon the greater variation in the candle-power of the light sources in the blue, as compared with the red (according to the temperature radiation law), since this would increase the heterochromatic as well as the homochromatic thresholds. The most probable explanation is that, possibly owing to the high refrangibility of the blue light, the subject's eye was unable to form a sharp image of the field upon the retina, so that the brightness gradient between the spatial components of the field was markedly decreased in the blue. This effect would be at a maximum in the comparison of blue with itself, or with violet. It is possible that bad definition at the boundary of the compared fields also decreases the influence of color difference upon the threshold—by diminishing color contrast—so that one and the same factor could operate to increase the homochromatic and to decrease the heterochromatic threshold.

It is of course evident from the qualitative data previously at hand that the heterochromatic factor must exhibit a minimum for a minimum of color difference between the standard and the comparison fields. However, except for this generalization, the form of the function connecting the threshold with degree of color difference has remained undetermined. Although considerable deviations from regularity are shown by the points of Fig. 7, they nevertheless fall upon what, for psycho-physical threshold data, may be regarded as fairly smooth curves. It will be seen that, in general, the threshold tends to increase in passing from the standard to the neighborhood of its *complementary*, beyond which it begins once more to decrease. However, the increase is larger for a given increment of color difference, when this is added to a previously given small difference, than when it is added to a difference which is already large. This is

especially apparent in the case of the curve for the red standard.

Unfortunately, Nutting's hue scale, as published, does not include the purples; if it did, the scale would obviously become cyclical. It is evident, also, that if the heterochromatic threshold measurements were extended over the whole cycle of hues, each of the curves of Fig. 7 would necessarily intersect the ordinate, unity, at two points, although these points would both represent the same hue.

The curve for the data obtained with the red standard is the most regular and continuous of the four, and may be selected for special study. The best representative smooth curve for these data, as plotted, does not differ a great deal from an arc of a circle having its center on the ordinate, unity at a point corresponding in the hue scale with a wavelength of about 500 $\mu\mu$. The equation for this circle, when expressed in terms of the plotted variables, is that of an ellipse:

$$\frac{(.61 - h)^2}{.61^2} + \frac{(1 - f)^2}{2.96^2} = 1, \tag{6}$$

where h is the relative position on the hue scale, and f is the value of the heterochromatic factor. This equation simplifies to

$$f = 1 + 2.96\sqrt{3.28\,h - 2.69\,h^2}. \tag{7}$$

For a value of $h = 1.22$, the curve again cuts the axis of the abscissæ, and for values larger than this f is an imaginary quantity.

Too much emphasis can easily be laid upon the significance of the above mathematics. The data are, of course, relatively crude for purposes of curve-fitting, and it is probable that factors of asymmetry have been introduced into all of the curves, on account of optical effects due to the variation in refrangibility of the stimuli form different parts of the spectrum. However, the general form of the relation between the differential threshold for brightness, and color difference, may be regarded as determined by the above reasoning.

The heterochromatic factor is roughly proportional to the number of just noticeable homochromatic luminosity steps

contained in a given heterochromatic step, if such an expression will be permitted. The exact number, n, of homochromatic steps equivalent to a given heterochromatic step can be found by use of the formula:

$$n = \frac{\log (1 - fm)}{\log (1 - m)}, \tag{8}$$

where f is the heterochromatic factor, and m is the relative (fractional) homochromatic threshold. This formula follows directly from the definitions of the various concepts which are involved. If the data of Fig. 7 were plotted in terms of n instead of f, the values of the ordinates would be slightly greater and the curves would be somewhat more elongate than is actually the case. For example, the n corresponding with the heterochromatic factor 4.00 (520 $\mu\mu$) for the red standard, is 4.38, and for the factor 2.96 (610, same standard), is 3.12. A graph using h and n as coördinates would be expressed entirely in terms of subjective scales of quality.

The feature of the above studies which is perhaps of the greatest practical importance for photometry is the relatively great influence exerted upon the brightness threshold by small color differences, such as are met with, for example, in the comparison of the light emitted by incandescent bodies at different 'color temperatures.' Assuming 134 just noticeable differences of hue in the entire spectrum, calculation based upon equation (7) shows that a threshold difference in hue should raise the heterochromatic factor from unity to 1.45.

RATE OF PUPILLARY DILATION AND CONTRACTION[1]

BY PRENTICE REEVES

In a recent determination of the least amount of radiation capable of giving rise to a perception it was necessary to know the diameter of the pupil when fully adapted to darkness.[2] The results in the literature were few and discordant,[3] so that it was decided to measure the pupils of the observers used in the experiment. In the general investigation which is being carried out in the Eastman Kodak Research Laboratory under the description of "Visual Sensitometry" the sensibility of the retina is examined throughout a range of light intensity from total darkness to a brightness obtained by reflecting full sunlight from white paper. For each brightness used the pupil has a certain diameter and the problem was to determine these values for several subjects. In parts of this study of retinal sensibility it was possible to use only one eye, the other being closed, and this introduced the problem of the effect of exposing one or both eyes to the given

[1] Communication No. 66 from the Research Laboratory of the Eastman Kodak Company. Paper read before the American Psychological Association at Pittsburgh, December, 1917.

[2] Prentice Reeves, 'Minimum Radiation Visually Perceptible.' *Astrophysical J.*, 1917, **46**, 167–174.

[3] T. H. Blakesley (*Phil. Mag.*, 1910, *29*, 966–969) reports a maximum diameter of 6.74 to 7.20 mm., Nutting says the pupil ranges from 2 mm. to 8 mm., though he measured only 6.9 mm. for a brightness of log − 5 millilamberts (*Trans. Illum. Eng. Soc.*, 1916, *11*, 1–21). Stevenson found an average maximum of 8.5 mm. with a mean variation of only 3 per cent. (*J. of Brit. Astron. Assn.*, 1916, **26**, 303). With a preliminary method comparable to that of Blakesley and Nutting the writer found an average maximum of 6.8 mm. for his own pupil while flashlight photographs gave a maximum of 8.3. All evidences seem to point to wide variations between observers as well as in the same individual's pupil from time to time, which seems to disagree with Stevenson's results. See Troland, Psychol. Rev., 1915, **22**, 167–176. Results by Langfeld show a small diameter (*Zsch. f. Sinnesphysiol.*, 1908, **42**, 349–358).

brightness. This is a study of the so-called consensual reflex.[1] The rate of change of sensibility when going from one brightness to another is still another part of the general research which involves a special consideration of the pupil. The change in sensibility, such as occurs when passing from a brightly lighted room to a darker one or vice versa, results from the operation of two factors, a retinal process (whatever it may be) and the change in the diameter of the pupil. The latter then calls for the determination of the rate of opening and closing of the pupil, that is, a quantitative determination of the pupillary reflex.

In the first part of this work, the investigation of the consensual reflex and the diameter of the fully dark adapted pupil, instantaneous photographs were taken with a 5 x 7 view camera. The subject was focused on the ground glass of the camera, fixed in a head rest, adapted to total darkness for at least fifteen minutes and then the flashlight taken. In the determination of the minimum radiation visually perceptible three observers were used who had pupillary maxima of 8.4, 8.3 and 7.4 mm. giving an average of 8.0 mm. A small millimeter scale was placed in the plane of the pupil so that it would be magnified to the same extent as the pupil and make the records easy to read. For two of these observers several series of photographs were taken over eight brightness levels including total darkness at one end and the reflection of full sunlight from white paper at the other end. The effect of exposing one or both eyes to the sensitizing brightness was studied throughout this entire range of intensities. The shape of the curves plotted for the two subjects is about the same, though the actual pupillary diameters show individual differences. Fig. 1 shows the curves obtained from one observer and we see that over the range of ordinary brightness the closing of one eye markedly increases the diameter of the pupil. Fig. 2 shows some of the photographs and the data plotted in the previous figure. We see from these results that

[1] Dunn (*Arch. of Ophth.*, 1917, 46, 193–210) says "when both eyes are normal, no matter what the difference of illumination before the two eyes, the two pupils remain of practically the same size—this is the result of what is called the consensual light reflex."

the decrease in the diameter of the pupil after the intensity increases beyond 100 millilamberts is slight so that in further experiments the maximum intensity used was in the vicinity of 100 ml. This gives a minimum diameter of the pupil near enough to the actual minimum and yet avoids the exposure of the subject's eyes to a highly unpleasa nt g are.

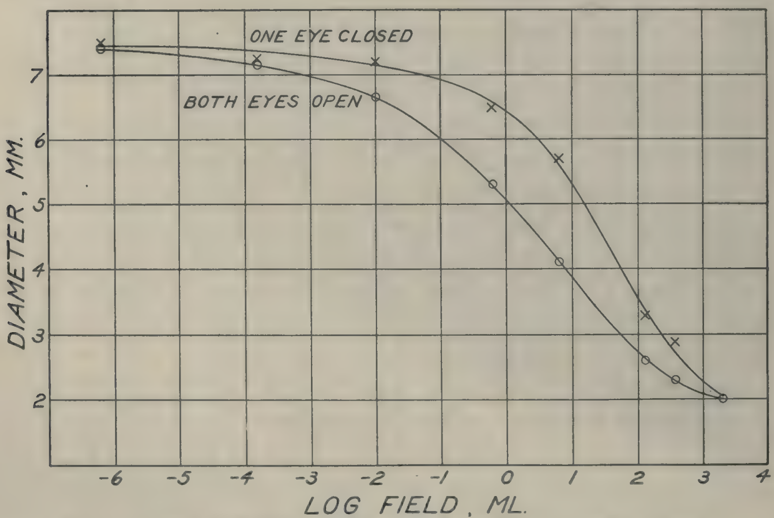
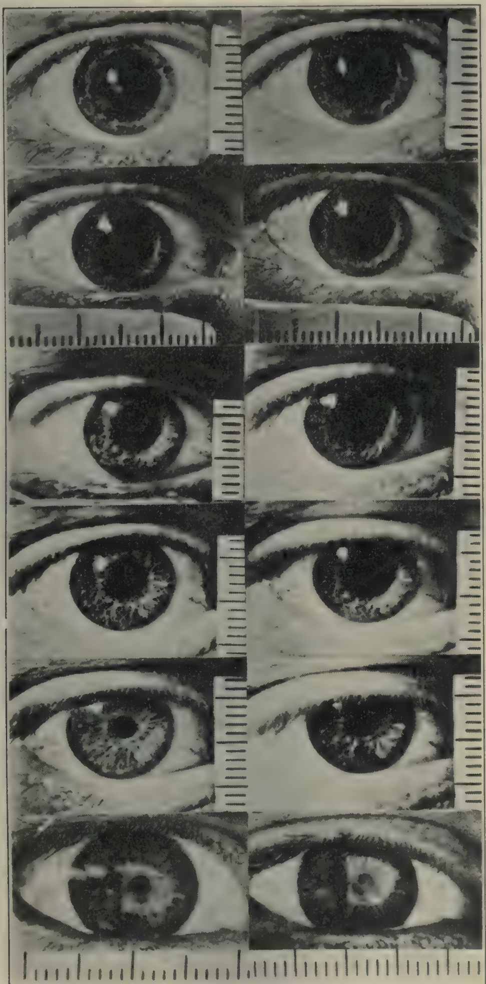


Fig. 1. Effect of Exposing One or Both Eyes on Diameter of the Pupil.

From these results six suitable brightness levels were chosen and the eyes of six other subjects were measured when both eyes were exposed. One of the first subjects was also used in this part of the experiment as a check on the method. A motion-picture camera was used in place of the view camera, and a bank of lamps replaced the flash powder. An extension was used on the lens so that a full-sized image was recorded on the film. After sufficient time for adaptation to the sensitizing brightness electrical connections, operated by the experimenter, started the motor driving the motion-picture camera to introduce unexposed film, the taking lights were turned on and the picture was taken. Several pictures were taken of each subject on different days and the results averaged. Fig. 3 shows the curves for six subjects with the

Both Eyes Open A | One Eye Closed B



Diameter Mm. A	Diameter Mm. B	Brightness Ml.
7.4	7.5	0.05
7.15	7.25	0.00015
5.3	6.5	0.60
4.1	5.7	6.3
2.6	3.3	126
2.0	2.0	2000

FIG. 2. Effect of Closing One or Both Eyes on Diameter of the Pupil.

average represented by the heavy curve. We notice individual differences in these curves and rather large variations in the maximum diameters of the pupils. Fig. 4 shows some

of the actual photographs taken and the data for the average of all seven subjects.

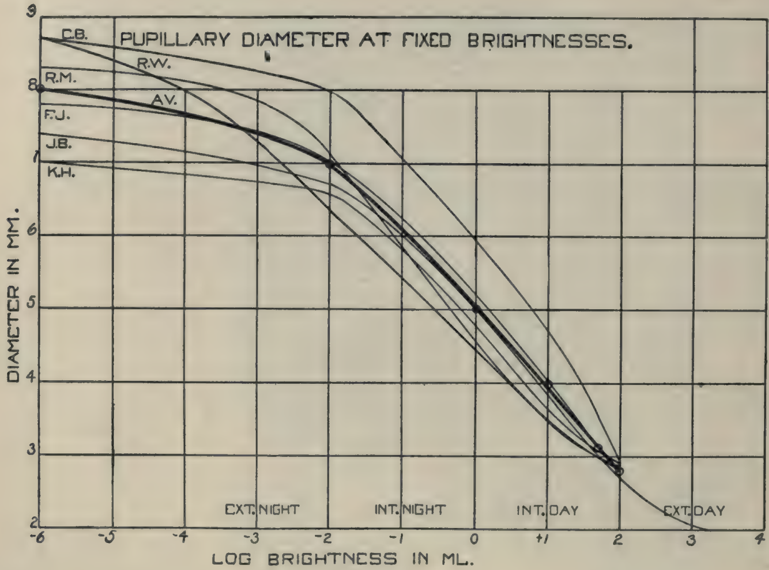


FIG. 3

J.B. C.B. K.H. F.J. R.M. R.W. P.R.

Av. diam.	Brightness
8.1	.0
7.2	0.1 Ml.
5.3	1.0 "
4.0	10.0 "
3.1	55. "
2.8	100. "

FIG. 4. Pupillary diameter at fixed brightness.

The rate of closing of the pupil was measured by taking motion pictures of an eye fully adapted to total darkness (so as to get the maximum pupil) as it closed to the minimum diameter chosen. By means of the electrical connections of the apparatus the motion-picture camera could be operated at a speed that gave pictures a tenth of a second apart so that, with the pupils readily measurable, it was an easy task to get the rate of closing. The previous six subjects were used as well as two others and Fig. 5 shows the curves from the six.

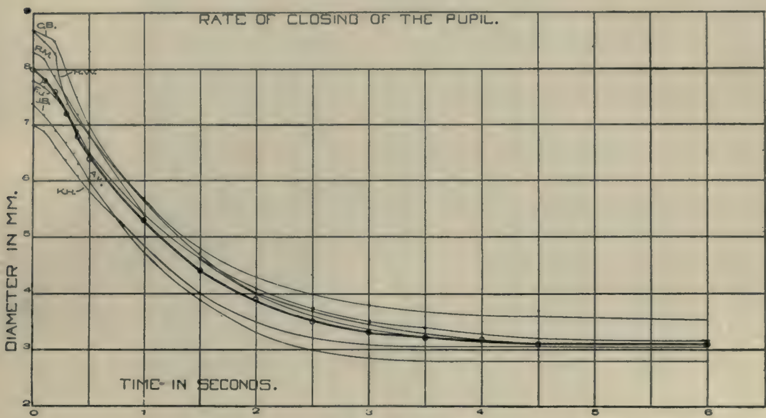


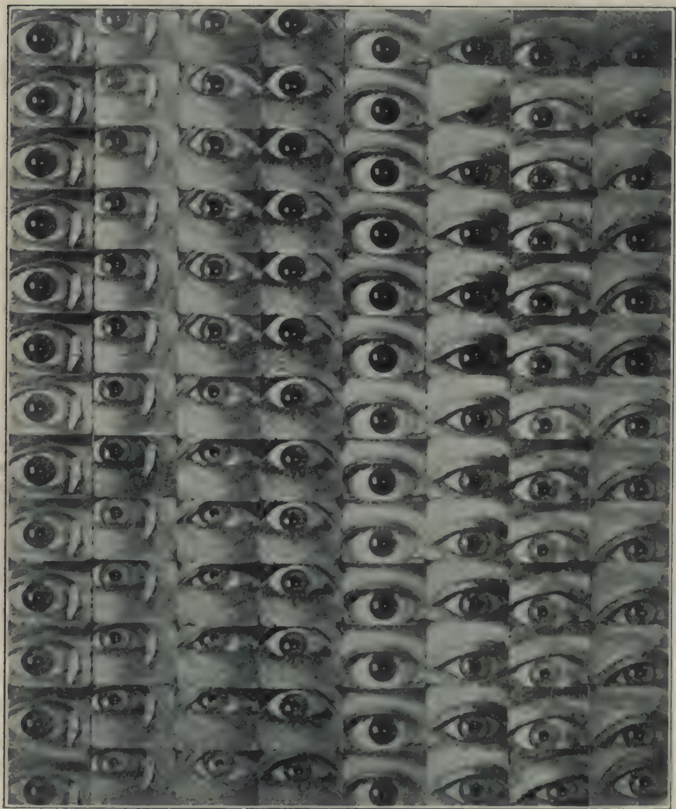
Fig. 5

It can be seen that the average pupil reached its minimum in less than five seconds, the greater part of the closing occurring within the first two seconds. Fig. 6 shows the photographs and average data from the eight subjects. An inspection of these photographs will show the variations between subjects.

In obtaining the rate of opening of the pupil a paradox presented itself, since we must have darkness to get the pupil to open but we must have light to get a photographic record. So an electrical device was made which turned off the lights for any desired time, started the motion-picture camera, again turned on the taking lights and photographically recorded the amount of opening after the given time in darkness. From a preliminary series suitable time intervals were chosen and the time and rate of opening of the pupil was obtained

for seven subjects. Fig. 7 shows the curves of the six subjects, and it is seen that the average pupil required from three to ten minutes to reach its maximum diameter from the initial diameter as chosen. Fig. 8 shows the photographs of

J.B. C.B. K.H. F.J. R.M. R.W. C.F. C.M.



Time	Av. diam.
0	8.1 mm.
.1 sec.	7.9 "
.2 "	7.7 "
.3 "	7.4 "
.4 "	7.0 "
.5 "	6.6 "
1.0 "	5.5 "
1.5 "	6.4 "
2.0 "	4.0 "
2.5 "	3.7 "
3.0 "	3.5 "
3.5 "	3.4 "
60.0 "	3.2 "

Fig. 6. Rate of closing of the pupil.

several stages in the process of opening. It is interesting to compare the rate of opening with the rate of closing; the time for opening to a maximum averages about five minutes as compared to the same number of seconds for closing.

It is a well-established fact that pupillary diameter varies

with the convergence, so in order to keep this factor constant a fixation point at a distance of 35 cm. was used. In darkness an illuminated pinhole was used, the intensity of which was kept at a just perceptible intensity for a few seconds just before the picture was to be taken by means of a variable rheostat controlled by the subject himself. Each subject

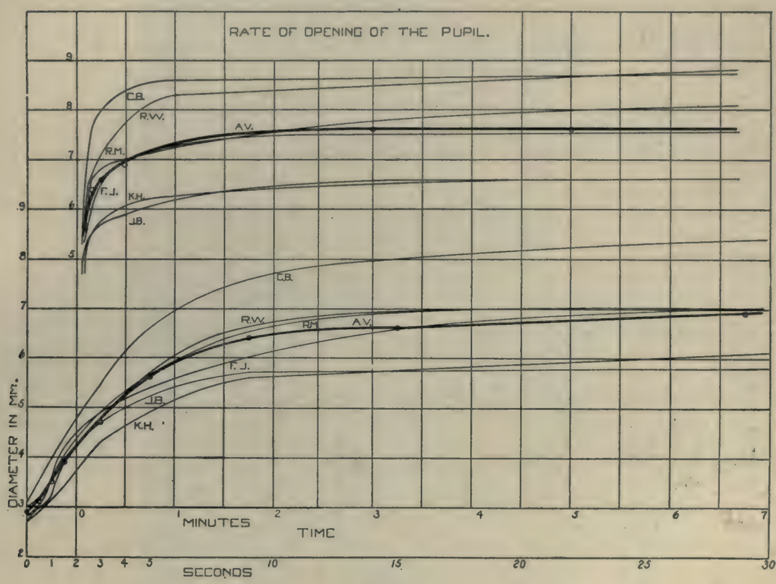


FIG. 7

was photographed at least four times for each of the curves plotted and some of them sat for as high as twenty series. The data plotted are averages of all series, and are shown in the four tables. Previous investigations of the pupil show that the color of the iris has no effect on its action, but that age is a factor.[1] In this study, however, none of the subjects were old enough to show a marked effect of age.

It must be remembered that in the results for the opening and closing of the pupil two definite brightnesses were used and that the time and rate of functioning of the pupillary reflex in changing from one brightness to the other is probably

[1] Kanngieser, F., *Arch. f. Augenhk.*, 1909, 63, 78–87. Langfeld, *op. cit.*

true for that change only. That is, if any other combination of intensities be used we could expect a different rate for the reflex action. Another thing to bear in mind is that practi-

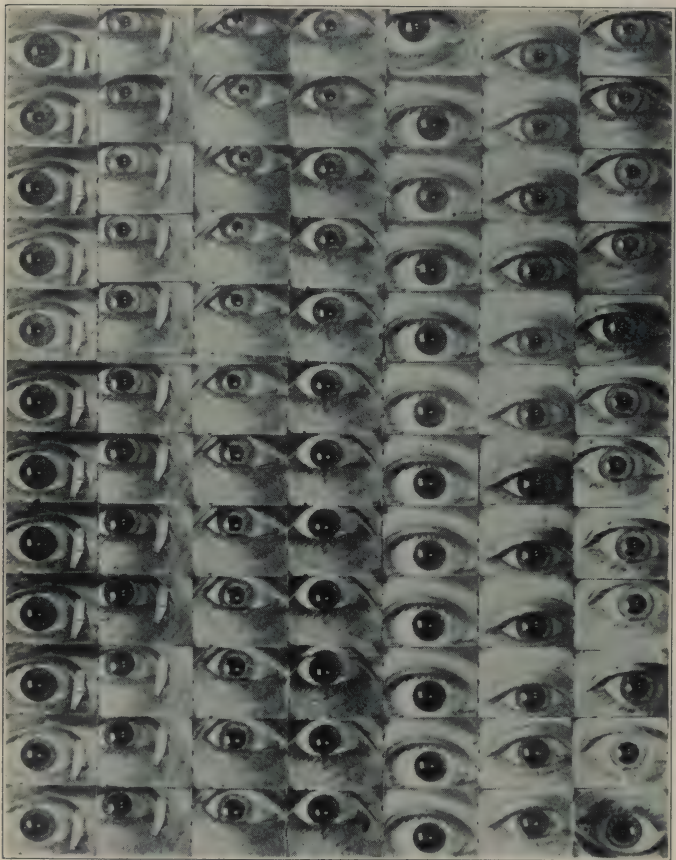


FIG. 8. Rate of opening of the pupil.

cally white light, tungsten filaments and sunlight, was used and that we might expect different results for colored light and especially for monochromàtic sources. In working out the time and rate of adaptation curves marked differences

were found for different colored lights and for different intensities of the same light. As the pupillary action is one part of the process of adaptation we can safely make the above

Table I

Effect of Closing One Eye on Pupillary Diameters

Brightness in Millilamberts	Log *B*	Both Eyes Exposed		One Eye Closed		Difference	
		J.B.	P.R.	J.B.	P.R.	J.B.	P.R.
0..........taken as	−6.0	7.4	8.6	7.5	8.6	0.1	0
0.00015............	−3.82	7.1	8.5	7.2	8.5	0.1	0
0.01...............	−2.0	6.7	8.3	7.2	8.5	0.5	0.2
0.6................	−0.22	5.3	7.0	6.5	7.8	1.2	0.8
6.3................	0.8	4.1	4.3	5.7	5.9	1.6	1.6
126.0..............	2.1	2.6	2.9	3.3	3.3	0.7	0.4
355................	2.55	2.3	2.4	2.9	2.9	0.6	0.5
2,000..............	3.3	2.0	1.9	2.0	2.0	0	0.1

Table II

Pupillary Diameters at Fixed Brightnesses

Brightness in Millilamberts	Log *B*	J.B.	C.B.	K.H.	F.J.	R.M.	R.W.	Average of 6	P. R.	Total Average
0......taken as	−6	7.4	8.7	7.0	7.8	8.3	8.7	8.0	8.6	8.1
0.00015........	−3.8	7.1	8.4	6.8	7.6	8.1	7.9	7.6	8.5	7.8
0.01...........	−2	6.7	8.0	6.7	7.0	7.1	6.3	7.0	8.3	7.2
1.0............	0	5.0	5.9	4.8	5.2	4.6	4.5	5.0	6.9	5.3
10.............	1.0	3.9	4.8	3.6	4.6	3.5	3.5	4.0	4.4	4.0
55.............	1.7	3.0	3.5	3.1	3.1	3.0	2.9	3.1	3.3	3.1
100............	2	2.7	2.9	2.7	2.9	2.8	2.9	2.8	2.9	2.8
2,000..........	3.3	2.0	—	—	—	—	—	2.0	1.9	1.9

Table III

Rate of Closing of the Pupil

Number of Picture	Time in Seconds	J.B.	C.B.	K.H.	F.J.	R.M.	R.W.	Average of 6	C.F.	C.M.	Total Average
1	0	7.4	8.7	7.0	7.8	8.3	8.7	8.0	8.0	8.6	8.1
2	.1	7.2	8.6	6.9	7.7	8.2	8.5	7.8	7.9	8.5	7.9
3	.2	6.9	8.5	6.6	7.5	7.9	8.3	7.6	7.8	8.4	7.7
4	.3	6.7	8.0	6.4	7.2	7.5	7.4	7.2	7.5	8.2	7.4
5	.4	6.3	7.5	6.1	6.9	7.2	7.1	6.8	7.2	7.9	7.0
6	.5	6.0	7.0	5.8	6.0	6.9	6.8	6.4	7.0	7.6	6.6
11	1.0	4.7	5.7	4.9	5.4	5.4	5.8	5.3	5.9	6.3	5.5
16	1.5	3.9	4.8	4.0	4.6	4.6	4.7	4.4	5.0	5.3	4.6
21	2.0	3.3	4.3	3.6	4.1	4.0	4.0	3.9	4.4	4.6	4.0
26	2.5	3.0	4.0	3.2	3.7	3.7	3.7	3.5	4.0	4.2	3.7
31	3.0	2.9	3.8	3.1	3.5	3.3	3.5	3.3	3.8	4.1	3.5
36	3.5	2.8	3.6	3.0	3.4	3.3	3.2	3.2	3.6	4.0	3.4
41	4.0	2.8	3.7	3.1	3.3	3.1	3.2	3.2	3.5	4.0	3.3
46	4.5	2.8	3.7	3.0	3.2	3.1	3.1	3.1	3.6	3.9	3.3
51	5.0	2.8	—	—	—	—	3.1	—	—	3.9	3.3
	1 min.	2.8	3.4	3.1	3.1	3.1	3.1	3.1	3.4	3.8	3.2

TABLE IV

RATE OF OPENING OF THE PUPIL

Time in Darkness	J.B.	C.B.	K.H.	F.J.	R.M.	R.W.	Average of 6	R.K.	Total Average
0	2.7	3.1	2.7	2.9	2.8	3.0	2.9	2.7	2.8
.5 sec.	3.0	3.5	2.9	3.1	3.0	3.2	3.1	3.0	3.1
1.0 "	3.2	4.0	3.2	3.6	3.6	3.7	3.5	3.5	3.5
1.5 "	3.9	4.4	3.4	4.2	3.9	3.8	3.9	3.9	3.9
3.0 "	4.7	5.3	4.3	4.7	5.0	4.4	4.7	4.3	4.7
5.0 "	5.3	6.6	4.9	5.4	5.8	5.7	5.6	4.9	5.5
9. "	5.7	7.6	5.6	6.0	6.6	6.7	6.4	5.4	6.2
15. "	5.7	8.0	5.5	6.6	6.9	6.9	6.6	6.1	6.5
29. "	5.8	8.4	6.1	7.0	7.0	7.0	6.9	6.2	6.8
1 min.	6.2	8.6	6.3	7.3	7.2	8.3	7.3	6.3	7.2
3 "	6.6	8.7	6.5	7.5	7.8	8.5	7.6	6.8	7.5
5 "	6.6	8.6	6.6	7.3	8.0	8.7	7.6	7.2	7.6
10 "	—	8.9	—	—	—	—	—	—	—

statements. This study has opened a large problem and much information awaits the treatment of other brightness combinations and the treatment of colored light sources.

The writer wishes to acknowledge the services of the members of the laboratory who so patiently acted as subjects in such a trying experiment. The writer is especially indebted to Dr. Julian Blanchard for his coöperation in the entire experiment.

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The Psychological Review

COMMUNICATION, CORRESPONDENCE AND CONSCIOUSNESS

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When a muscle of a man's body contracts, the operation is due, as almost every reader knows, to a molecular disturbance that is propagated through a hair-like nerve fiber from the brain or spinal cord. He knows also that similar impulses travel from the sense organs to the brain. It seems to be commonly believed, however, that the brain can originate some of these movement-exciting impulses. Prevailing methods of education have led most men to look at mental processes from the subjective side and to judge of the thoughts and feelings of others as like their own. At this date we can find but a small number of men who have given sufficient study to mechanistic theories of thought processes to be able to discuss them intelligently. Among those who have studied these matters, we find men who admit that reflex action is a case of nerve mechanism but do not concede that reason and purpose are similarly constituted. The general idea is, we may say, that at one end of the scale we have the simple mental processes which are nervous impulses in operation and at the other end we have the highly complex mental processes, presided over by the inner consciousness.

The question then arises as we approach the upper end of the scale, what part is taken by the nervous mechanisms? When consciousness is most complete, what is occurring in the nerve fibers? The answer to this question may be taken as the goal towards which we will work in this discussion.

When a boy can faithfully describe in language his present environment and recent changes in his environment, we can say that he has a conscious mind. In such a case we may assume that the real environment of the boy is known to ourselves, the observers, as we can change it at will and as our minds are more fully developed than his. From our knowledge of physiology we know that his consciousness is in some measure dependent on operations in his brain and nervous system. Let us then consider briefly these operations.

We know that when a feature of the environment is in communication with a part of the brain that there is first an outer communication of the feature with a sensory nerve ending and second an inner communication from the nerve ending to the brain. We may look at the matter as if there were an outer environment made up of real objects and an inner environment representing the outer environment and located at the nerve endings. For example a vibrating bell is of the outer environment but the sound vibrations in the inner ear are of the inner environment. We may then think of the environment of an individual as two zones. Let us call them the 'near zone,' which is at the nerve endings, and the 'far zone,' which is all outside. We will now take up the near zone as acting on the brain.

There are special nerve endings in the skin for heat and others for pain. It is known that if the nerve fiber is cut, there is no longer any sensation of heat when the nerve ending is heated in one case and no sensation of pain when the pain receptor is stimulated in the other. In order that there be a sensation, the nervous impulse must reach the brain. It seems probable that the propagated impulse from a pain receptor is very much like one from a heat receptor. How then does the brain discriminate between the heat signal and the pain signal? We may find a parallel case in the telephone instrument. Electric impulses pass over the wire and one impulse is like another and yet we get widely different sounds from the receiver. We can readily distinguish the sound of the fife from that of the drum. The sound in the receiver

simulates the sound in the transmitter. On looking into the case we find that by the mechanism of the transmitter the sound waves are transformed into electric impulses and by the mechanism of the receiver the electric impulses are transformed back into sound waves. Let us consider if similar transformations take place within the nervous system. We find that by the mechanism of the ear sound waves are transformed into nervous impulses. We find likewise that light, heat and electricity are transformed at the proper receptors into nervous impulses. By the organs of taste and smell, chemical action is converted into nervous impulses. Dynamic force also, of course, is converted into nervous impulses. All of these transformations we know are made with the organs of sense.

The question then arises, can these transformations be reversed? Can nervous impulses be transformed into sound, light, heat or electricity or into chemical action or dynamic force? We find that they can be, provided suitable mechanisms exist. Nervous impulses produce sound in the vocal apparatus just as well as electric impulses produce sound in the telephone receiver. There is little doubt that in the lightning bug there is an apparatus by which nervous impulses cause light, and that in the electric eel there is an apparatus by which nervous impulses cause electricity. Without going into this matter more fully, let us consider it settled that by suitable mechanisms, nervous impulses can be transformed back into all the familiar forms of energy. It follows then that if there should be suitable mechanisms located at the brain centers, the nervous impulse would be converted into heat for example, or into other forms of energy. The next question for us to consider then is this: Have we any reason to suppose that there are mechanisms in the brain centers which can transform nervous impulses back into the form of heat, etc.? Our knowledge of the physiology of the brain cells is very limited. We find there is some reason to think that chemical change, heat and movement at least, occur in the cells. One might argue also, that if there can be movement produced in a cell,

something like a sound wave might be caused. One might argue also that light and heat are near of kin.

As a result of this brief survey of the matter we may conclude that it is conceivable that nervous impulses are, at the brain centers, transformed back into sound, heat, etc., but it is not probable. It is thought fair to assume, however, that afferent impulses are at the brain centers transformed to some extent into different forms of molecular movement. We may assume also that there is a certain form of movement for sound and another for heat and another for pain, etc. There is then, we will say, a mechanism at the brain center that acts like a telephone receiver and transforms the afferent impulses into a form of molecular movement that in effect simulates the stimulus at the sense organ. It is by aid of this central mechanism then that the brain discriminates between the heat signal and the signal of some other sense.

We may suppose that this central mechanism is something on the same plan as we assume to be in efferent nerve endings at the effectors of the body, such as a tear gland for example. Or, looking at it another way, we may suppose it as the obverse of the mechanism at the receptor where the nervous impulse originates. In other words we will assume that in the case of a signal from a heat receptor, there is a molecular disturbance at the proper brain center which is the same as if the heat were applied to the brain center itself. In the case of vision we will assume that the molecular change at the brain center is the same as it would be if a ray of light should penetrate it. In the case of sound, the change at the brain center is the same as if the sound vibrations reached it. In the case of taste, a drop of acid on a taste receptor makes a change at the brain center the same as if the acid were applied there. In smell the same rule will hold.

In other words in all afferent impulse cases there is a change in some brain center that in effect reverses the process that occurs at the nerve ending. In short whatever transpires at the near zone transpires in effect, as we will assume, at appropriate brain centers. Let us carry the idea out and suppose that a kinæsthetic impulse from a moving

muscle to the brain causes a molecular movement in the proper brain center that is very much like the change taking place in the substance of the muscle. So the change in the muscle does in effect take place also in the brain center. Let us carry the idea out farther and suppose that the process is reversed for all cases of efferent impulses from the brain to the effectors. There is a change or movement in the brain center that is simulated in the muscle or gland at the nerve ending. Considering a whole circuit, we see that if a pain receptor is stimulated there will be a wound effect process at the brain center which will provoke a molecular movement that sends an efferent impulse to a muscle which will have a simulating molecular movement that will cause the muscle to contract.

It is true of course that in exceptional cases the change in the brain center does not simulate the change at the nerve ending, as when a pressure on the eyeball gives a sensation of light. We may pass them by as too exceptional to be of consequence in this discussion.

To proceed: let us now think of the effect of the environment on a creature of the lowest intelligence. Let us assume a tadpole-like animal on a sandy beach. It will have rudiments of a head, of a tail and of one leg on each side. It may be surrounded by sand, by water or by air, or be partly in one, partly in another element. It has a cerebral ganglion that we will call the brain. In the brain there will be a certain area for the tail and an area for each leg. When the tail is in the water, certain brain fibers will be made active; when it is in the sand, other fibers will be aroused. The environment of each limb will be manifested in the brain by the activity of certain nerve fibers. So for every situation there will be a simple simulating process that is characteristic. As already shown it is allowable to assume that the disturbance in the brain is an echo of the disturbance at the leg or tail. The brain is then so constituted that it automatically adjusts itself to the environment. Of course in the human brain, the operations are much more complex.

As a sort of starting point for this discussion let us pred-

icate that in a creature having a cerebral cortex, if a unit feature of the environment is represented at the near zone, there will be communication to certain nerve fibers in a sensory area of the brain and there will be a unit change in those fibers which simulates in effect the change in the near zone. Of course in life there are always many features simultaneously represented in the near zone and therefore acting together upon the brain in question. It helps us to get a clear idea, however, to think of a unit feature as a cause and a unit nerve change as an effect. Whatever that is present at the near zone is simulated in effect in the nerve centers. As between the near zone and the brain we have communication and simulation and we may believe that the process is a simple one. We shall find that the far zone is also simulated to some extent in the cortical centers, but, as we shall see later, the process is much more complex. An understanding of how the near zone is simulated helps us to understand how the far zone is simulated.

It is true, of course, that the sense organs or more especially the distance receptors are highly developed mechanisms of communication between the far zone and the near zone of the environment. We may consider that the functions of these mechanisms are understood and have been explained by physiology. Rays of light and waves of sound are by these means utilized for communication. We shall see later how association mechanisms provide for the accompanying simulation of the far zone.

When a child is conscious of its environment, there is a disturbance in its brain that, as we shall see, simulates the far zone. The child is conscious of what it touches, what it sees, what it hears, what it smells, what it tastes. That part of the far zone which is in active communication with the brain is simulated by the disturbance in the brain centers. The child is not conscious of a change in its ear drum or retina. If the child is listening to a phonograph the sound is located at the phonograph and not in the ear. When the child is conscious of an object there is a communication between the object and the child's brain. There is also, as above stated,

a simulation of the object in the brain. These are two important operations, we see, communication and simulation.

To make the point clearer, let us say that *AA* is a feature of the far zone that is in communication with our subject. Let *Aa* be the feature as represented at the near zone. Let *A* be the change that takes place in the nerve fibers due to the excitation coming from *Aa*. In the same way let *BB* be another feature of the far zone, *Bb* be the feature as represented at the near zone, and *B* be the effect on the nerve fibers. Let *CC* be a third feature of the far zone and *Cc* the same at the near zone and *C* be the effect and so on. The case can be shown by a simple diagram as follows:

Environment Far Zone	Near Zone	Cortical Centers
AA	*Aa*	*A*
BB	*Bb*	*B*
CC	*Cc*	*C*
DD	*Dd*	*D*

The dotted lines represent the paths of communication between the environment and the brain centers.

For the sake of clearness and brevity in this discussion, let us use the term 'mimetic process' to express the operations that occur in the brain of an individual when he is conscious of his present environment. Let us also use the term 'image process' to express the operations of his brain when he is conscious of his past environment.

To proceed: let us think of a man and of his shadow on the wall. As the man moves, the shadow moves. We see that there is a correspondence between the shadow and the real man. Now think of a child observing the man. There is a pattern of the man in the child's brain something like the shadow on the wall. If the man moves there is a change in the brain disturbance.

To illustrate the idea more precisely, let us think of a case in photography. We will suppose a dimly lighted furnished room and in it a double camera for stereoscope pictures. We adjust the focus, put in a pair of sensitive plates and open the shutters. Owing to the dim light a long-time exposure is needed to make a good negative. If

we were now able to observe the change going on in the sensitive film, we would find a correspondence between the molecular or chemical change and the features of the room. Let us for convenience use the term 'film process' to express the change that is taking place in the film. We may say that the film process corresponds with the environment. We can almost say that the film is conscious of the environment.

Now if the lenses are good and their focus is right, the correspondence will be good. So we see that the degree of correspondence depends upon the quality of the lenses and the correctness of the adjustment. For want of a better expression, let us use the term 'degree of verity' to express the correspondence of the film process in the two plates with the features of the room as they actually are. If we close the shutters, the film process ceases. As communication is cut off the correspondence stops. It is then a case of communication with simulation.

To compare with this let us think of a child looking into another furnished room. There will be in the child's brain a molecular change that corresponds with the view of the room. This change is the mimetic process. It is something like the film process in the camera plates. The degree of verity in the mimetic process will depend upon the correctness of vision, the illumination and other matters to be considered later.

Let us remember the scientific principle that like causes produce like effects. In the case of the double camera, a like environment will give a like film process. In the case of the child, a like environment will give a like mimetic process. Thus do we get correspondence of the brain process to the situation. Remember also that the child is not conscious of the changes in the organ of vision or brain centers, but only of the things represented by the mimetic process.

To get an idea of the image process, let us take a different case and think of a child that is observing a man in the first instance. There is a corresponding mimetic process in the child's brain. The child is conscious of the man. At another time suppose that, from some cause, the same process occurs

completely in the child's brain although the man is not present. The child is conscious of the man when there is no man. This, then, is an illusion. When the child has a memory image of the man the brain process is not entirely complete as in the case of an illusion. We may say that a memory image is, as it were, a partial illusion. The incomplete brain process is, as we have said, the image process. Or to take a different view, let us say that when the child has a memory image of the man, there is a secondary brain process that simulates the original mimetic process and so indirectly simulates the object (man) seen in the past. This secondary process is the image process.

In further illustration of these matters, let us think of a child taking a long walk. As he proceeds along the path, he is conscious of the changes in the environment as they are presented. It will sometimes happen, however, that the child is conscious of the environment at a point some distance back on the path instead of that where he is walking at the moment. This will usually occur when the child observes some feature of the road that resembles one that he has met at the point further back. The sight of the third mile post may cause him to recall a gateway that he saw at the second mile post. We have then an image process, we may say, that simulates a past environment although there is now no direct communication between that environment and the brain. This is the work of nervous mechanisms, or perhaps of nerve-muscle mechanisms.

To explain more definitely, we may say that the sight of the third mile post provokes a short series of changes in the nerve-muscle system that produces afferent impulses similar to those caused by the sight of the gateway at the second mile post. In this way is the child made conscious of his past environment. Observe that the child is not conscious of any changes in his nerves or muscles. He is conscious of the gateway, now a thing of the past.

By way of further illustration and explanation, think of looking at a picture in a stereoscope. You do not see the two prints, you seem to see real objects in perspective. By

means of the devices peculiar to the instrument, an artificial mimetic process is provoked in your brain centers and the illusion is perfect. You are thus made conscious of an environment as though you saw it in reality. You see a monument, for example, where in reality there is no monument. It is the same way with a memory image. For instance, you are conscious of a scene of boyhood days. Some association fibers in your brain have been stimulated as though by the device of the stereoscope and for a moment you are conscious of the past scene.

It is the same way with the child in our other illustration. Ask the child what he has seen along the road and he will describe the gateway, showing that he is conscious in some degree of features in the environment that he passed through. In order that the child may recall the gateway, he must have been conscious of the gateway before and he must have mechanisms of associative memory that produce a stimulation of the same brain centers as were affected by observing the gateway.

On consideration we find that when a child is conscious of its present environment it is because the environment is in direct communication with the child's brain and there is a resulting correspondence of the brain processes with the environment. We also find that when the child is conscious of a past environment, it is because some stimulus has provoked a recurrence or simulation of brain processes that were previously produced by that environment.

We can make the whole matter clearer by the use of symbols. Let AA at the far zone and Aa at the near zone be a feature of the environment that was in communication with our subject in the first instance. Let A be the change in the cortical fibers due to Aa and let a be a change that is similar to A which comes at a later time and is provoked by an association fiber. Other features of the far zone will be BB, CC and DD, represented at the near zone by Bb, Cc and Dd. The correspondence nerve changes at the time will be B, C and D, and the subsequent changes will be b, c and d. By a simple diagram we have:

Environment			
Far Zone	Near Zone	Mimetic Process	Image Process
AA	*Aa*	*A*	*a*
BB	*Bb*	*B*	*b*
CC	*Cc*	*C*	*c*
DD	*Dd*	*D*	*d*

Let us remember that it is the object that makes the mimetic process and that it is the object that is known to the conscious subject.

Looking further into the matter, we find a great variation in the degree of verity, *i. e.*, in the degree of precision and definiteness of the correspondence of the brain processes with the actual environment. There is good reason to think that the verity increases with training, and education.[1] We know that a man notices objects that a child does not and that an educated man notices objects that would escape the observation of an uneducated man. Think of an engineer and a shoemaker looking at a steam pumping engine. The brain correspondence of the former will be much more precise and definite. On the other hand the savage sees the track of a wild animal that is invisible to a white man.

The increasing degree of verity of the mimetic process that comes from training is partly due to better communication from the environment to the brain, especially in the case of vision. The fixation and focusing of the eyes is a matter of practise. Discrimination with any of the senses increases with training. On consideration we see that the degree of verity keeps pace with the formation of memory associations, *i. e.*, with the growth of habits. After an object has become familiar to you, you can no longer see it as you did at first sight. We may say, then, that the degree of verity of the mimetic process is largely determined by mechanisms of associative memory.[2] To put it another way, unless the association nerve paths have been prepared by previous experience, the brain process will not simulate the environment. Knowledge of the present environment is dependent on associative memory. When you look up the street on a

[1] James R. Angell, 'Psychology,' Holt, New York, 1908, p. 176.

[2] W. B. Pillsbury, 'The Fundamentals of Psychology,' Macmillan, New York, 1916, 341, 342.

bright day and are conscious of trees in the foreground and of a church spire in the distance, your power of coördinating the scene is due to physical conditions and to a long course of training and experience.

It may help you to realize this if you take a new magazine and look at the pictures upside down. Many of them will appear meaningless. To an untrained mind, a dog's for example, the pictures would be equally meaningless when right side up. We may believe that in the untrained mind the correspondence is a confused one. It may be something like one's impressions while looking at a four-ring circus.

In this connection we may observe that the reason a boy is conscious of the words of one speaker only in a room where others are talking, is because the mechanisms of associative memory make up the mimetic processes that correspond with the words of the one. The speaker that he hears is usually the one he has been watching.

For the purposes of this discussion we will say that association nerve fibers are dual common paths, each having two tributary connections that are sometimes excited in sequence and thus the common path is made more open for impulses that control behavior. So when the child is conscious of an object, we know that the object is exciting certain nerve receptors and it transpires that a volley of impulses must be following a certain group of association fibers (common paths) that have been developed by previous operations.[1] The greater the number of these fibers that take part and the more highly developed they are by previous operations, the greater will be the degree of verity.

We can only judge of a creature's intelligence by its behavior. We cannot be sure that a child is conscious of its environment unless it gives expression to its feelings by some response. It is not until a child has learned to talk that we know much about his mental activities. We say a child is observing because he gives expression to impulses provoked by his environment. On the other hand if a child does not notice changes in his environment, we say he is unconscious.

[1] Angell, *op. cit.*, p. 169.

It follows then that our discussion should include some consideration of expression and of its relation to correspondence and consciousness. We may find that the employment of language increases the degree of verity. Suppose a child is observing a squad of soldiers. You ask him to count them. When he has done so and answers 'Five', you may know that his brain correspondence has increased. At the same time that a child is learning to talk, he is learning to give expression to his experience. There can be little doubt that this sort of training has an important effect in raising the degree of verity in the child's brain processes. By the time the child can name every object in the nursery, he has acquired a high degree of verity with that particular environment. In the same way when he learns to draw from a model, the effect is to raise the degree of verity with visible objects. Let us note that learning to talk or draw pictures is the development of mechanisms of associative memory. It is the result of training. The studies of mathematics, weights and measures and geometry must have an important effect in raising the degree of verity of the mimetic processes. When a group of children are sight-seeing together, they remark on what attracts their attention. Thus they acquire habits of expression which affect future mimetic processes to an important extent. It is safe to say that in the minds of many men, thinking is only incipient talking, as it were, for at least the greater part of the time. We find then that language is clearly an important factor in the development of brain correspondences.

We have already remarked that the coördination of mimetic and image processes is a matter of associative memory. In young people the correspondence of the consciousness with past and present environment is an increasing one. New concepts are constantly being acquired. We may suppose by way of illustration that the first clear correspondence is with the environment of the nursery. The child has a concept of the room and its furnishings that is independent of the sensations of the moment. No matter what direction the child faces, the room still seems to him the same place. In

consequence of a vast number of associations, the concept has been established. Having this concept to start with, new concepts are joined to it, so that by gradations the house, the yard and the street are annexed in the mimetic processes. The field grows wider and wider, taking in the school, the church and the park, etc.

When a man returns to his home and recognizes it, there is a particular group of association fibers "wakened up" in his brain. When he enters his room if he is conscious of the room, there is a particular group of nerve fibers wakened up, no matter which door of the room he enters by. The mechanisms by which this is done have been built up step by step. We may say that in recalling a certain room, there are incipient movements made that would actually be made in observing the features of the room and in giving expression to them by word of mouth, or, if one is an artist, by drawing or painting them. To look at it another way, a concept of a certain room is a composite picture of all one's past experiences in that room. Each experience developed certain common nerve paths which are now in use.

To look at it again from a little different angle, we may say that the first time an object is seen, attention movements are made. The object is examined, felt, smelt, tasted, viewed, named, etc. When it has become familiar, all the association fibers that were developed in getting acquainted with the object become aroused at the sight of the object. In the same way each familiar feature of a certain room tends to arouse a group of fibers in the cortex.

When the environment is one that is continuously changing, the changes are simulated in the brain centers, so that we have a mimetic process of the changing situation. We can also have an image process of a former changing environment. In such a case it will be found that the degree of verity is a matter of training and experience. When you hear words spoken in your own tongue, you can easily repeat them. If they are in a foreign tongue, you can scarcely do so. An image process of a changing environment that has high degree verity is no doubt the joint operation of mechanisms of

associative memory. It is safe to say that the concept of time is built up of memory associations connected with change and movement.

In this way we are brought to the conclusion that the coördination of mimetic processes so as to create a correspondence with the world as it really is, is due to the development of association nerve mechanisms or habit mechanisms. When the nerve mechanisms are highly developed, each point in the observed environment is in communication with a particular area in the brain that acts as an annunciator, as it were. Any change in the environment provokes an annunciating process in a certain area of the brain.

Let us now note that when communication is cut off the potential correspondence does not vanish. It gradually but rapidly diminishes, rapidly at first and then more and more slowly. You can recall the environment of a few seconds ago quite clearly, but that of an hour ago is by no means as clear. When a man has an image process of a past environment it is referred to in speech as arousing a memory image. As we have already noted, the image is an incomplete illusion. Let us remember that the image grows fainter and fainter with the lapse of time. In a book of recent date the author argues that the image is dependent upon motor processes. She states: "The basis of a train of mental images . . . must be the excitation of a train of motor responses."[1] There is much to be said in support of this hypothesis.

The memory image process accompanies the operation of association mechanisms. From our point of view it is the partial resurrection of a former mimetic process. A series of memory images is in many cases a resurrection of a series of mimetic processes in a former experience. There is a tendency for image processes to succeed each other in the same order as the experiences to which they correspond.

It is an interesting question whether the image process can be said to cause effective motor impulses in some cases. It is not quite safe, from our point of view, to say that it can.

[1] Washburn, M. F., 'Movement and Mental Imagery,' Houghton Mifflin Co., Boston & New York, 1916, p. 49.

It may well be that the image process is of no greater direct utility than a rainbow, an echo or the wake of a steamer. We believe, however, that a certain image process may always be followed by a certain movement and it is all the business of association mechanisms. Let us take a sort of middle course and say that the image process in some cases takes part in provoking movement. On the other hand, it is very probable that kinæsthetic impulses from movements or incipient movements take an important part in arousing image processes.

It goes without saying that association mechanisms determine the selection of image processes. The provoking impulses tend to follow those nerve paths that have been left open by recent or frequent previous impulses. In other words the selection of image processes is a matter of habit.[1]

A child's thoughts probably run on the things he has been doing with and the things he has been talking about. As he observes one thing at a time, so does he recall one thing at a time. As he talks about different things in order, so does he recall different things in order and thus confusion of image processes is usually avoided. Of course an image process of yesterday's scene will tend to crowd out one of day before yesterday.

Owing to the great number of association paths, each image process helps to provoke others and so they occur in turn without cessation like a continuous show, except when some interruption produces a mimetic process.

Up to this point we have considered the image process as in correspondence with a past environment. It is true of course that an image process often shows a close correspondence with an environment that transpires later on. We may explain this briefly by saying that a man knows the future by the past, so the image process that anticipates the event is composed of elements derived from earlier mimetic processes. Language habits and education are important factors in such cases. Children can be taught to make new designs and to invent combinations of objects. In a previous article

[1] Angell, *op. cit.*, p. 207.

the writer has shown that purposive behavior can be explained in terms of mechanisms of associative memory.[1] Expectation in image processes can be explained in the same terms, for the same reasons, it is thought.

Let us now take a brief review of our demonstrations. In the beginning we found that a unit feature of the near zone of the environment in active communication with a certain brain area causes a unit nerve change which in effect simulates the feature. The typical mimetic process is constituted in part of such unit nerve changes and in part of changes in association nerve fibers developed by former experience. The nerve fibers used in language and expression play an important part in the mimetic process. The image process is constituted of nerve changes which are the offspring of those in the mimetic process which it simulates. We found that the association mechanisms used in language and expression are important factors in perception. We found that it is largely by association mechanisms that the present environment is simulated in the brain and that the selection of image processes is due to association mechanisms. It follows that the continuity of consciousness is also due to such mechanisms.

In conclusion we may assert that a man's consciousness relates primarily to his environment and depends upon mechanisms for communication and mechanisms of correspondence. The latter are mimetic processes and image processes which are the action of complex nerve mechanisms composed largely of mechanisms of associative memory. The writer has elsewhere demonstrated that associative memory can be explained by the dual common path theory.[2] We need not consider the theory here, but we note that association nerve fibers form paths of varying conductivity for the propagation of nervous impulses.[3] It follows that mimetic processes and image processes are coördinated movements of impulses in connected association nerve fibers in communication, in the

[1] Psychol. Rev., 1917, 24, p. 413.

[2] Psychol. Rev., 1916, 23, p. 235.

[3] The conductivity varies in proportion to the frequency and recency of previous passages.

first instance, with the environment. Just as the blood rushes into a boy's cheeks when he is embarrassed, so do the nervous impulses rush into certain areas of branching nerve paths in the cerebral cortex at the proper signal from the environment. The degree of verity depends upon the extent the association fibers have been developed by previous nervous discharges and upon the number of association fibers that are aroused. These brain processes are of course directly or indirectly provoked by afferent impulses from the organs of sense and often occur in conjunction with effective motor impulses. A conscious voluntary movement is due to effective impulses from the cortex to the muscles in coördination with a mimetic process provoked by the environment.

On consideration we find that when consciousness is most complete there is a brain process having a high degree of verity and composed of coördinated movements of impulses in groups of association nerve fibers. There is adequate communication from the object to the brain and accurate correspondence of the brain process with the object.

On the whole it appears that the developing brain is a highly multiplicate piece of machinery which in its daily period of activity is continually matching itself with some part of the grand total of its past and present environment with a fidelity that increases from time to time.

THE HETEROCHROMATIC DIFFERENTIAL THRESHOLD FOR BRIGHTNESS: THEORETICAL[1]

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VI. Discussion of Results: *B.* The Heterochromatic Brightness Threshold in Relation to Psychological Color Systems

The Bearing of the Results on the Doctrine of Psychological Primary Colors.—The four standards used in the work described in the first installment of the present paper were selected purposely so as to coincide approximately with the spectral positions of the four so-called *psychological primaries*, which form the basis of the Hering, Ladd-Franklin, and other theories of color vision. The direct determinations of the wave-lengths for these primaries made by Westphal,[2] were utilized, although it was necessary to employ simple spectral red in place of spectral red mixed with a slight amount of blue, as demanded by Westphal's results.

It is the opinion of the writer that the specification of these four colors as primaries is not arbitrary, as is claimed by Von Kries, Wundt and others; and can be given a logical justification on the grounds of an analysis of the relations of similarity and difference existing between the members of the hue series. Such analysis indicates that the psychological primaries occupy positions at which critical changes occur in the mode of variation of hue with respect to wave-length, or with respect to hue scale number. In other words, at these points something takes place which is equivalent either to a change of sign of a differential of quality, or to a sudden shift in the qualitative units in which this differential, or derivative, must be expressed.

[1] From the Nela Research Laboratory of the General Electric Co., Cleveland, Ohio.

[2] Westphal, H., 'Unmittelbare Bestimmungen der Urfarben,' *Zeitschrift für Sinnesphysiologie*, 1910, 44, 182–230.

If, as the Hering theory demands, all possible hues in trichromatic vision are to be regarded as results of the *fusion in various proportions* of non-antagonistic pairs, selected from four primary chromatic qualities, the question of color difference or similarity divides itself into a study (1) of the differences between the primaries, as such, and (2) of the degree of coparticipation of any primary or primaries in a given pair of colors. The distance which separates any two colors on the hue scale would thus be but one of a number of factors determining the actual dissimilarity of the colors in question, as it would be proportional to this dissimilarity only within the range between two neighboring psychological primaries.

TABLE VII

VALUES OF THE THRESHOLD, t, AND ITS VARIATION, v, FOR ALL PAIRS OF PSYCHOLOGICAL PRIMARIES, WITH CORRECTIONS FOR 'SPACE ERROR'

Color Pair	Subject T.		Subject L.	
	t	v	t	v
BG..........	.126	.056	.134	.062
GB..........	.159	.032	.072	.019
Mean........	.143	.044	.103	.041
Error........	.016	.012	.031	.022
BY..........	.118	.030	.141	.030
YB..........	.201	.042	.147	.060
Mean........	.160	.036	.144	.045
Error........	.042	.006	.003	.015
BR..........	.173	.039	.164	.059
RB..........	.195	.062	.199	.028
Mean........	.184	.051	.182	.044
Error........	.011	.012	.018	.017
GY..........	.173	.044	.103	.033
YG..........	.185	.041	.148	.030
Mean........	.179	.043	.126	.032
Error........	.006	.002	.023	.002
GR..........	.192	.044	.231	.037
RG..........	.214	.040	.144	.026
Mean........	.203	.042	.188	.032
Error........	.011	.002	.044	.006
YR..........	.150	.038	.199	.066
RY..........	.194	.052	.135	.032
Mean........	.172	.045	.167	.049
Error........	.022	.007	.032	.017

Table VII gives the values of the thresholds for all pairs of primaries, each as a standard and as a comparison color. 'Space errors' (see above) and the values of the thresholds, as corrected for these 'errors,' are also given. The corrected

thresholds for both subjects are arranged in order of magnitude in Table VIII., together with the corresponding variation measures. The same table also gives the sizes of the intervals between the primary pairs in the hue scale, in order of magnitude.

It will be noticed that a fairly close correspondence exists between the order of the hue intervals and that of the thresholds, for both subjects, although there is very little

Table VIII

Corrected Values of the Thresholds and Variations, for Pairs of Primary Colors, for Both Subjects, Arranged in Order of Magnitude; Together with Hue Scale Intervals of the Same Pairs, Similarly Arranged

Subject *T.*		Subject *L.*		Hue Scale Intervals
t	*v*	*t*	*v*	
BG, .143	BY, .036	BG, .103	GY, .032	BG, .175
BY, .160	GR, .042	GY, .126	GR, .032	GY, .255
YR, .172	GY, .043	BY, .144	BG, .041	YR, .325
GY, .179	BG, .044	YR, .167	BR, .044	BY, .430
BR, .184	YR, .045	BR, .182	BY, .045	GR, .580
GR, .203	BR, .051	GR, .188	YR, .049	BR, .755

correspondence in the case of the variation measures. However, that linear separation of qualities in the hue scale cannot be taken as an adequate measure of color difference is obvious at once from the cyclical relation of the hues. Red and violet, for example, resemble each other more closely than do red and green, yet the former pair of colors has a separation on the hue scale of 1.00 whereas the latter has only 0.59. Taking into consideration the cyclical relation of the hues, it can be seen that the only order which the hue scale differences definitely demand of the threshold values in Table VIII. is: BG, GY, YR, $\frac{\text{GR}}{\text{BY}}$. The only significant exception from this order for either subject is in the case of BY. The reversal of GY and YR for subject T (GY, .179; YR, .172) is readily attributable to chance variation.

The average value of the threshold between all psychological primaries is .1733 for subject T. and .1515 for subject L. The average values of the *homochromatic* thresholds for the

two subjects are .0478 and .0403, respectively, the corresponding average heterochromatic comparison factors being 3.626 and 3.760. The average of variation measures, *v*, for the primary pairs is .0478 for subject T. and .0402 for subject L.; for the homochromatic pairs: .0115, and .0103 respectively; the corresponding factors being 3.766 and 3.902.

It is conceivable that the heterochromatic threshold or comparison factor will provide us with a measure of the actual magnitude of the color difference (or similarity) between two visual qualities, and it is possible that in this capacity they may throw some light not only on the nature of differences due to different degrees of participation of a common primary or primaries in two qualities, but also upon the relations of difference and similarity which must be supposed to hold between the primaries themselves. Although red and green are antagonistic, it is not immediately obvious that red is more dissimilar to green than it is to blue, although introspection seems to confirm the opinion that red and yellow and blue and green form pairs, the members of which possess bonds of resemblance.

Examination of Fig. 7 shows that the exact position of the maximum of the heterochromatic factor, plotted with respect to the color difference between the standard and comparison fields, is not entirely unambiguous. In the case of the red standard the most probable position of the maximum appears to be in the blue-green, which is the complementary of the standard, but in the case of the yellow standard, the maximum seems to be in the green, rather than in the blue. Since the complementary of green is purple, we should not expect to find the maximum for this standard represented in our data; and the drop in the violet, for the green, is probably due to adventitious dioptric influences.

The contents of Table VIII. suggest partial answers to some of the questions raised above. In order to study them more in detail, the average values of the heterochromatic comparison factor were computed for the following cases: (1) for all 'antagonistic' pairs of primary qualities, (2) for all 'neighboring' pairs of primary qualities, (3) for all qualities

in which the psychological primary hue of the standard does not participate, (4) for 'homothermal' pairs (YR, BG), and (5) for 'heterothermal' pairs (BY, GY, BR, GR). The results are shown in Table IX., first, including the data from the blue standard and, second, omitting these data on account of their doubtful significance. It will be seen from the table that

TABLE IX

THE HETEROCHROMATIC FACTOR AVERAGED FOR DIFFERENT CLASSES OF QUALITIES (SEE TEXT)

Class of Qualities	Including Blue			Excluding Blue		
	Subject T.	Subject L.	Mean	Subject T.	Subject L.	Mean
Antagonistic pairs of primaries...	3.60	5.10	4.35	5.27	6.04	5.66
Neighboring pairs of primaries...	3.93	4.01	3.97	4.57	4.54	4.56
Non-coparticipating pairs.......	3.77	4.41	4.09	4.60	5.38	4.99
Homothermal pairs of primaries..	3.68	3.82	3.75	4.35	4.37	4.36
Heterothermal pairs of primaries..	4.39	4.66	4.52	5.04	5.38	5.21

the average value of f is considerably lower for the neighboring pairs than it is for the antagonistic pairs, the difference being still more marked if the data from the blue standard are neglected. The ratio between the two is 0.913 with the data from the blue standard, and 0.806 without these data. The average value of f for the third case mentioned above, lies between those for the first two cases. This would seem to indicate that antagonistic primaries differ intrinsically more than do non-antagonistic primaries. However, the difference between the average heterochromatic factor for the two cases is not large, and is perhaps increased illegitimately by the fact that the red standard—being a spectral red—contained a slight yellowish component.

The grouping of the primary hues into 'warm' and 'cold' pairs, on the basis of direct inspection, appears also to be supported by the results shown in Table IX., since the average value of the heterochromatic factor for all homothermal pairs is 3.75 (or 4.36) as compared with 4.52 (or 5.21) for all heterothermal pairs.

In considering these results it should be borne in mind, not only that the use of the heterochromatic factor as an

index of absolute color difference is hypothetical, but that the subjective or experienced color quality of the standard cannot be regarded as being strictly constant in our experiments, on account of the influence of *color contrast.* Such contrast would tend to move the hue of the standard along the hue scale in a direction opposite to that taken by the variation of the comparison color. At present, there are no data available which can be used to correct for, or to evaluate this effect. In general, it would tend to make the actual hue differences greater than those represented in Fig. 7, and would compel us to regard the position chosen for the standard in the hue scale as being merely its approximate mean position for a complete set of comparisons around the color cycle. In the present work, since the comparison sets omit the purples, the mean positions will be displaced—except possibly in the case of the green standard—towards the end of the spectrum to which the given standard is in closest proximity.

The Theory of the Influence of Color Difference on the Brightness Threshold.—In a previous paper[1] the writer has suggested, as a general principle, the statement that 'the distinctness of any experiential (or qualitative) dimension changes in parallel with the degree of similarity of two compared experiences in all other dimensions.' For example, if we consider the quality of any visual sensation as being determined by its position in an ideal three-dimensional scale, having the coördinates, hue, saturation, and luminosity, the magnitude of the just noticeable difference in hue would possess a minimum for equal luminosities of compared sensations; or, *vice versa*, the just noticeable difference in luminosity would have a minimum—as shown in our experiments—for identical hue values.

If we represent these dimensions geometrically, it can easily be seen that an analogous relationship exists in the case of the discrimination of spatial positions or levels. Suppose that *a* is a fixed point in a plane, that *b* is a variable position, and that we are required by simple observation to determine the positions of *b* at which it appears to be just noticeably

[1] The paper previously cited, J. of Exp. Psychol., 1917, 2, 18.

above an imaginary horizontal axis drawn through a. It is self-evident that the deviation of these positions from the true horizontal will be greater the greater the separation of a from b along the axis in question. The position of this locus of thresholds will be determined by at least two factors: (1) the space threshold, or visual acuity, which holds when a and b are on the same vertical, and (2) the certainty with which the 'eye' can establish an imaginal horizontal line with respect to which to estimate the position of b.

It would be a reasonable hypothesis to suppose that the mean variation of the position of the imaginal axis is a constant when expressed in *angular* terms, *i. e.*, that the axis acts like an inflexible line swinging about a as a fulcrum. If this mean angular variation is ϕ, and if d is the separation of b from the vertical axis, the mean linear variation of the position of the axis, in the vertical passing through b, must be $d \tan \phi$. If the acuity threshold is u, the magnitude of the threshold for vertical displacement should be in accordance with the accepted theory of 'propagated errors.'

$$v = \sqrt{u^2 + d^2 \tan^2 \phi}. \tag{9}$$

Schemes such as the conventional color triangle or color pyramid, which attempt to give a geometrical representation of a two or three-dimensional scale of experiential qualities, are constructed on the tacit assumption that the relations between the qualitative dimensions are quantitatively the same as those of the axes and units of the space system. Although this is by no means a logical necessity, it may prove of some interest to work out certain elementary consequences of this assumption in its application to the theory of the heterochromatic factor. The natural unit of measurement along all axes of a qualitative system is, of course, the just noticeable difference, or sensory threshold.

The system of visual qualities is represented by Titchener, and others, by means of a double pyramid.[1] The equator of this pyramid encloses a quadrilateral plane figure, the apices of which are the positions of the four psychological primaries.

[1] Titchener, E. B., 'A Text-Book of Psychology,' 1910, 63.

The purpose of this construction is to represent the 'linear' character of the variation in hue occurring in the intervals between primaries, and the sudden alterations in the mode of variation, which appear at the primaries. If the construction is valid, the separation of any two hues in the hue scale must be taken to represent the distance between them, measured along the 'equator' of the pyramid, while their 'true' difference would be given by the length of a straight line joining their positions. Such a straight line would coincide with the 'equator' only when both of the hues in question lay between the same two psychological primaries.

However, very simple considerations show that the representation of the relations of the spectral colors by means of a quadrilateral figure with rectilinear boundaries, cannot be accepted as mathematically correct. Such a diagram requires that the primaries should have a greater degree of saturation than have intermediate hues. Even if this were the case for the hues as given in the spectrum, it would be impossible to accept the relationship as a condition of the differentiation of the hues in general, since all of the hues may conceivably exist in equal saturation, without losing their distinctive character. However, the requirement is not even satisfied by the spectral colors, since—as shown by the laws of color mixture and color flicker—the saturation of these colors increases continuously from the middle of the spectrum towards either extreme. In the light of these considerations it must be deemed necessary to replace the quadrilateral by a figure with curving boundaries. The exact form of this figure for the spectral colors has not been determined, although it is probably somewhat similar to the ordinary color-triangle plot of the spectral hues.

If the figure in question were a circle—as it would be for a series of equally saturated colors—the value of the 'true' difference, d, between any two colors (see equation 9) in terms of the hue scale difference, h, would be

$$d = 2s \sin(h/2s), \tag{10}$$

where s is the radius of the circle—or the saturation of the

colors represented upon it—and the angle is expressed in radians. This is the usual formula for a chord of a circle. When combined with equation (9) this gives, for the heterochromatic factor,

$$f = \sqrt{1 - k \sin^2\left(\frac{h}{2s}\right)}, \tag{11}$$

If $h = 0$ or $2\pi s$, $f = 1$; if $h = \pi s$, $f = \sqrt{1 - k}$, which is its maximum value. The latter relationship permits us to write

$$k = f_{max}^2 - 1, \tag{12}$$

so that if we take $f_{max} = 5$, $k = 24$. The resulting curve is plotted in Fig. 8. This will be seen to satisfy the typical set

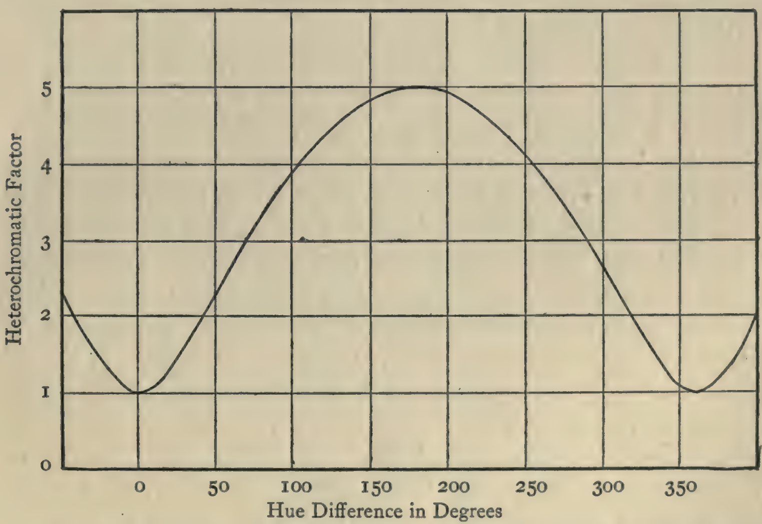

FIG. 8. Theoretical relationship between the heterochromatic factor and hue difference.

of data as regards general form. However, even if the geometrical analogies are all valid, complete agreement should not be expected, since our data are for spectral, rather than for equally saturated colors.

It will be noted that the above argument virtually em-

[1] The writer acknowledges his indebtedness to Dr. P. W. Cobb for his suggestive criticisms of this theory.

ploys an angular scale of hues, with a unit equal to $H/2\pi$ threshold units, where H is the total number of threshold differences in the color circle. Absolute hue is thus made a sort of trigonometrical or periodic function of hue difference, so that the hue for $h = 2\pi$ is the same as that for $h = 0$, and so on, assuming an axis of coördinates through any fixed point in the circle.[1] This use of circular notation to symbolize the relations of the hues appears to be more in harmony with the facts in the case than does the employment of linear symbolism. At present, however, it is an open question whether the relation, $H = 2\pi s$, which is implied by the spatial color diagram, actually holds for a cycle of equally saturated colors, when H and s are both measured by the threshold method.

The general theory of the elevation of the luminosity threshold with increase in the color difference between the compared fields, above suggested, is obviously consistent with the statement that the qualitative dimensions of luminosity and hue are to a certain extent *indeterminate*, since the axes with respect to which definite luminosities and hues are measured are not rigidly fixed in the system, but oscillate about a mean position. This mean variation in the angular position of the axes represents diagrammatically our uncertainty whether we are making a luminosity comparison, a hue comparison, or a comparison of saturations. It is only when the actual luminosity difference exceeds, by a just noticeable difference, the variability of the coördinates, that a definite judgment can be made. The origin of coördinates in such comparisons, is taken through one of the compared qualities.

As the writer has previously pointed out, this conception, if valid, has an immediate application to the controversy concerning the relative merits of the 'direct comparison' and 'flicker' photometers. In the flicker photometer, color difference is eliminated by fusion, so that the judgment is between successive qualities having the same color, but dif-

[1] I find that criticisms and suggestions, with regard to the dimensions of the color solid, very similar to the above, have been made by H. C. Warren, in a paper presented to the American Psychological Association in 1909, and abstracted in the Proceedings of the Association, Psychol. Bull., 1910, 7, 51–52.

ferent luminosities. In 'direct' or simultaneous comparison, the dimension of luminosity (as a concept) becomes inherently unstable. Moreover, different observers—as experience has shown—will establish their coördinates for the discrimination of luminosities, in different 'directions' in the color scale, some with a tilt towards the blue or towards white, others with a tilt towards the red. The present writer, for example, tends to overestimate yellow in comparison with other colors, apparently because yellowishness is to a certain extent involved in his concept of luminosity.

This uncertainty in the definition of luminosity, in the presence of a color difference, is expressed very well by Helmholtz, who says:[1] "I scarcely trust my judgment upon the equivalence of the heterochromatic brightnesses, at any rate upon greater and smaller in extreme cases. I admit, however, that one can gradually so darken one of two colored fields that no doubt remains as to the other being now the brighter . . . . As far as my own senses are concerned I have the impression that in heterochromatic luminosity equations it is not a question of the comparison of one magnitude, but of the combination of two, brightness and color-glow (*Farbengluth*), for which I do not know how to form any simple sum, and which too I cannot further define in scientific terms."

The above suggestions concerning the theory of heterochromatic differential threshold for brightness are of course very tentative, and should be supplemented by an hypothesis stated in physiological terms. Unfortunately, however, our knowledge of the physico-chemical processes which underlie discrimination, regarded as a nervous function, is practically nil. Before these problems can be attacked, we must at least possess a tenable theory of the manner of conduction of intensity and quality to the centers, a problem which at present remains unsolved.

[1] Helmholtz, H. von, 'Handbuch der physiologischen Optik,' IIte Auflage, 1896, 440. The translation is that given by J. H. Parsons, in his 'Introduction to the Study of Color Vision,' 1915, 43.

VII. Discussion of Results: *C*. The Relation between the Threshold and its Variation Measure

The properties of the variation measures, v, of the measurements reported above are of both theoretical and practical interest. These measures are given in Tables III. and IV., and are plotted in Figs. 3 to 8, inclusive.

Relative Precisions of the Threshold 'Points.'—In order to ascertain the relative precisions with which the several 'points' determining the threshold can be found, the fractional mean variations of each of the four points were averaged for all colors compared with each standard. The results are given in Table X. It will be perceived that, in the general average for all standards and all comparison colors, both subjects exhibit the same order in the arrangement of the points according to precision, viz., *a*, *b*, *c*, *d* (see page 318 above). From the values shown in Table X., it appears that, at least

Table X

Average Relative Precision of the Determining 'Points' of the Threshold (Fractional Mean Variation) for Two Subjects, and All Measurements Taken at 25 Photons

Case	Subject T.	Subject L.	Av. T. and L.
a (j. n. brighter)	.0341	.0233	.0267
b (j. n. n. brighter)	.0377	.0303	.0340
c (j. n. darker)	.0391	.0367	.0379
d (j. n. n. darker)	.0385	.0314	.0350
Av. of *a* and *c* (j. n. d.)	.0366	.0300	.0333
Av. of *b* and *d* (j. n. n. d.)	.0381	.0309	.0345
Av. of *a* and *d* (increase)	.0363	.0274	.0319
Av. of *b* and *c* (decrease)	.0384	.0335	.0360
Av. of *a* and *b* (brighter)	.0359	.0268	.0314
Av. of *c* and *d* (darker)	.0388	.0341	.0365

under the conditions of experimentation here described, an *increase* in the brightness of a variable field compared with a constant is more readily detected than a decrease; but that a judgment of equality (just not noticeably different) has nearly the same accuracy as a judgment of difference (just noticeably different). There is, however, a slight tendency in favor of the difference judgment. 'Just noticeably brighter' is the most accurate judgment of all; 'just noticeably darker'

the least accurate. The order of accuracy is represented conveniently by the numbers in the following diagram $\begin{matrix} b2 & \Big| \uparrow & 1a \\ c4 & \downarrow \Big| & 3d \end{matrix}$ the direction of the arrows indicating the direction of change of the variable brightness as the judgment was being made.

The Correlation between v and t.—It is obvious from Tables III. and IV. that v tends to increase as t increases, and that the relation between them is roughly linear. In order to throw more light upon this relationship, each value of t was divided by the corresponding value of v. The resulting quotients are given in Table XI. The general average of t/v for subject T. is 4.012 (m.v. = 0.61), and for subject L, 4.09 (m.v. = 1.04). Assuming the relationship to be linear, this would mean that, approximately, $t = 4v$, or the threshold as found by the method of limits is equal to four times the average

TABLE XI

VALUES OF THE RATIO, t/v (SEE TEXT), FOR TWO SUBJECTS, T. AND L., FOUR STANDARD COLORS, AND THIRTEEN COMPARISON COLORS, AT 25 PHOTONS

Comparison Color, μμ	Standard Color, μμ			
	Blue, 475	Green, 505	Yellow, 575	Red, 693
430, (T.).....	6.30	4.06	3.66	3.26
(L.).....	3.91	2.55	4.13	2.70
460, (T.).....	3.87	4.18	6.33	4.97
(L.).....	3.42	3.91	3.65	3.65
475, (T.).....	5.58	4.93	4.79	3.14
(L.).....	4.91	2.77	2.47	7.12
490, (T.).....	3.72	4.15	2.75	3.88
(L.).....	6.95	3.33	5.00	3.20
505, (T.).....	2.24	3.88	4.46	4.99
(L.).....	2.16	6.64	4.88	5.47
520, (T.).....	2.82	4.25	4.88	5.08
(L.).....	4.01	2.83	3.40	3.49
550, (T.).....	2.96	4.89	3.54	4.10
(L.).....	5.65	3.38	2.80	2.97
575, (T.).....	3.98	3.96	3.78	3.73
(L.).....	4.73	3.08	3.65	4.27
580, (T.).....	2.47	4.15	4.25	3.49
(L.).....	3.49	4.30	3.97	4.08
610, (T.).....	3.89	4.02	3.58	3.64
(L.).....	5.63	4.04	2.59	4.09
640, (T.).....	3.35	3.76	4.25	3.35
(L.).....	4.02	7.30	2.72	3.51
670, (T.).....	2.94	4.39	5.38	2.94
(L.).....	5.91	8.23	3.29	3.70
693, (T.).....	4.45	4.36	3.92	2.99
(L.).....	2.80	6.28	2.84	2.89

mean variation of its determining points. Diffusion plots of the actual values are given in Figs. 9 and 10.

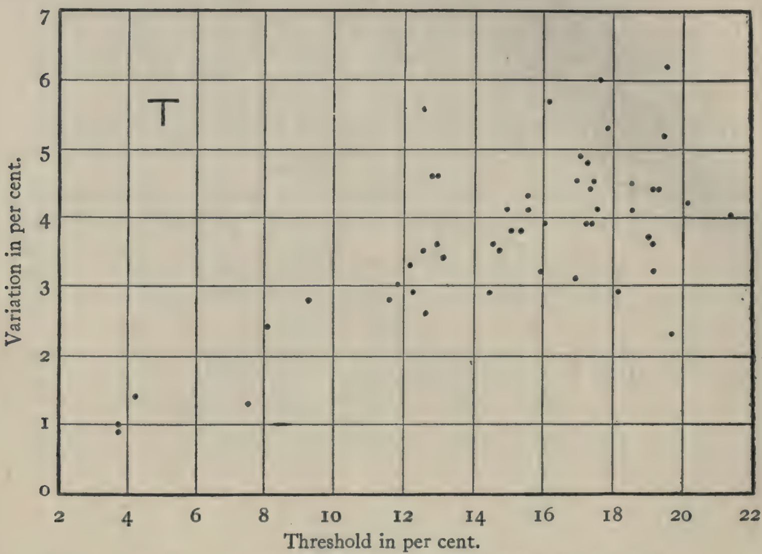

FIG. 9. Diffusion plot of threshold and variation values for subject T.

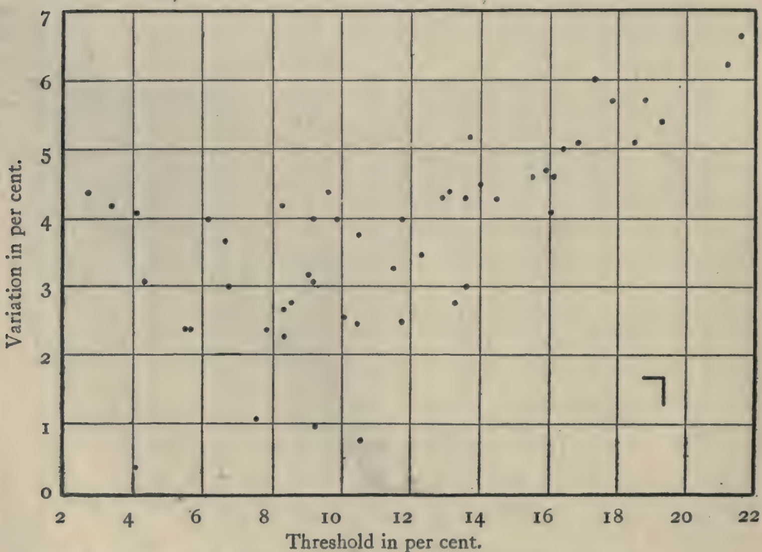

FIG. 10. Diffusion plot of threshold and variation values for subject L.

As a further study of this relationship, coefficients of correlation between t and t/v, were calculated by the conventional 'product-moments' formula. The results are given in Table XII. It will be perceived that for subject T. the

TABLE XII

CORRELATION RATIOS, r, BETWEEN t AND t/v, FOR FOUR STANDARD COLORS, AND TWO SUBJECTS, T. AND L.

Standard Color, $\mu\mu$	Correlation Ratio, r		P. E. of r	
	Subject T.	Subject L.	Subject T.	Subject L.
Blue, 475	− .0212	− .104	.187	.185
Green, 505	+ .281	+ .693	.172	.097
Yellow, 575	+ .341	+ .0850	.165	.187
Red, 693	+ .588	+ .526	.122	.135
Algebraic sum	+1.188	+1.200		
Algebraic average	+ .297	+ .300		

correlation is positive, and large, in the red, and decreases in passing along the spectrum in such a manner as to assume a slight negative value in the blue. The average value of the coefficient, .297, indicates that, in general, t/v tends to increase with increase in t,[1] so that the curve representing the actual function connecting t and v must be somewhat concave with respect to the axis of t, being represented approximately by the formula, $t = k\ v^q$ where $q > 1$. However, q decreases as we pass from the red to the blue end of the spectrum, and becomes less than unity in the blue.

The correlation values for subject L exhibit a tendency similar to those shown by the values for subject T, although less clear cut. The average value for L is .300, practically the same as that for T.

Mean Variation of the Threshold Values.—The mean variation of the determining points of the threshold is of course to be distinguished from that of the threshold itself. The latter can be calculated from the former, by means of the theory of propagated errors, application of which to the pro-

[1] If $y = f(x)$, $y/f(x) = 1$, and the correlation between a variable and a constant, such as unity, is zero. Hence, if we divide each value of y by $f(x)$, using the corresponding x, the coefficient of correlation between y and $y/f(x)$ should not differ significantly from zero, if we have chosen the right function.

blem gives the following formula[1] for the numerical mean variation, Δt, of the threshold in terms of the numerical mean variations, Δi and Δj, of the quantities, i and j in equation (5):

$$\Delta t = \frac{1}{2}\sqrt{\frac{1}{ij}\left[\left(\frac{j}{i}\Delta i\right)^2 + (\Delta j)^2\right]}. \tag{13}$$

For the purpose of our present calculations the difference in magnitude between $\Delta i/i$ and $\Delta j/j$ may be neglected, so that each of these quantities may be set equal to u. This permits us to rewrite (13)

$$\Delta t = \frac{u}{2}\sqrt{2j/i}. \tag{14}$$

But, from the definition of the threshold, $h/i = j/h = 1 - t$, where $h = \sqrt{ij}$ is a constant; so that $j = h(1 - t)$ and $i = h/(1 - t)$, whence:

$$\Delta t = \frac{1 - t}{\sqrt{2}} u. \tag{15}$$

In our calculations, i and j are the geometrical means of pairs of values contained in the formulæ: $i = \sqrt{ab}$, and $j = \sqrt{cd}$. By the theory of propagated errors:

$$\Delta i = \frac{1}{2}\sqrt{\frac{1}{ab}[(b\,\Delta a)^2 + (a\,\Delta b)^2]}, \tag{16}$$

but if $\Delta a/a = \Delta b/b = v$, we have, from (16)

$$\Delta i = \frac{v}{\sqrt{2}}\sqrt{ab}, \tag{17}$$

or $\Delta i/i = v/\sqrt{2} = u$. Hence we have for the numerical mean variation of the (fractional) threshold, as determined by the method of limits (using four points):

$$\Delta t = \frac{1}{\sqrt{n}}\,\frac{1 - t}{2} v, \tag{18}$$

where n is the number of measurements made on each of the four points, assuming the same number to have been made on

[1] This relationship was developed by differentiating the expression, $t = (i - \sqrt{ij})/i$, with respect to i and with respect to j, separately; multiplying each derivative by the corresponding m.v. and taking the square root of the sum of the squares of these products. This gives the most probable value of the m.v. of t.

each, and v is the average fractional mean variation of the individual determinations in each of the sets.

The data tabulated above show that, on the average, $v = t/4$, and that the deviation from this proportionality, for different heterochromatic comparisons, as tested by correlation ratios, is not great. Substituting this value of v in (18), we have:

$$\Delta t = \frac{1}{8\sqrt{n}} t(1 - t), \tag{19}$$

or, for the fractional variation,

$$\Delta t/t = \frac{1}{8\sqrt{n}}(1 - t). \tag{20}$$

For subject T., $n = 10$ (or more); for subject L., $n = 5$; so that the average fractional variation of the threshold for T. would be 0.040 $(1 - t)$, and for L., 0.056 $(1 - t)$. The largest value of t, for subject T. is .214, for which $\Delta t/t = .031$; the corresponding value for L. being .043. In other words, the mean variations of the average threshold values given in Table III. are in the neighborhood of three or four per cent.

In connection with this discussion it is of interest to consider the precision with which the value of $h = \sqrt{ij}$ can be determined, *i. e.*, the accuracy obtainable in heterochromatic photometry by direct comparison, when instead of attempting to equate two luminosities, we determine the upper and lower points for threshold difference between the standard and measured brightnesses.

By analogy with equation (17):

$$\Delta h/h = u/\sqrt{2}, \tag{21}$$

where u is the fractional mean variation of i and j. But $u = v/\sqrt{2}$, and $v = t/4$, so that

$$\Delta h/h = t/8 = .125t, \tag{22}$$

when one determination is made of each of the four points.

It would be interesting to compare this result with the precision obtainable by the method of random setting to apparent equality, using the same number of determinations.

This comparison could be carried out by the use of data obtained by the method in question under conditions similar to those of the above experiments, or in terms of calculated values based upon the data already at hand. Considerations of space forbid discussion of this problem in the present paper, but the writer hopes in a later article to treat this and allied questions concerning the statistics and precision of photometric measurements, in detail.

VIII. Summary

The purpose of the measurements discussed in the present paper was to determine the differential threshold for brightness as a function of the color difference existing between the compared photometric fields.

Values of the relative threshold with their variation measures were obtained for two subjects, using four standard spectral colors—the psychological primaries—and thirteen comparison colors—distributed over the entire spectrum—at an intensity of 25 photons. A supplementary series using one standard was carried out, for one subject, at an intensity of 240 photons. The 'method of limits' was employed.

Values of the 'heterochromatic comparison factor' or the ratio between the heterochromatic and the corresponding homochromatic thresholds, are calculated for all of the measurements made at 25 photons. This factor varies from unity, for no color difference, to a maximum of five to ten, for a maximum of color difference. The function connecting the heterochromatic factor with the color difference, expressed in just noticeable hue steps, represents approximately a segment of an ellipse.

In considering the bearing of the results upon the doctrine of psychological primary colors, it is found that, on the average, the value of the heterochromatic factor is greater for pairs of 'antagonistic' colors than for non-antagonistic pairs, and greater for pairs of 'warm' with 'cold' colors than for 'warm' with 'warm,' or 'cold' with 'cold.' Other interesting groups are studied in a similar way.

A discussion of the results in their relation to the 'color

pyramid' is presented, together with a general psychological theory to explain the influence of color difference upon luminosity discrimination.

Special attention is given to the variation measures of the four 'points' determining the threshold values. The relation between the average mean variation of these four points and the corresponding threshold value is found to be approximately a proportionality, the variation being approximately one quarter of the threshold. The deviations of this relationship from proportionality are studied, for the various standards, by means of correlation ratios.

The probable mean variations of the final threshold results are also calculated and shown to lie between three and four per cent.

ASSOCIATIVE AIDS: III. THEIR RELATION TO THE THEORY OF THOUGHT AND TO METHODOLOGY IN PSYCHOLOGY[1]

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V

We may consider the learning of associated pairs under yet another aspect, namely as problems to be solved by thought. That these are simple, but nevertheless real problems, is evidenced by the following facts: Most of them required more than one prompting, many of the responses were misplaced, sometimes responses of the subject's own construction were supplied, in the first few days the responses were slow and uncertain, and many of them were connected with their proper stimuli by ingenious associations of the subject's own device. That is, the learning of a pair involved an analytic function in that the pairs had to be separated from each other and a synthetic function in that two particular words or syllables, and no other, had to be connected.

The problematic character of the learning of these pairs is well illustrated by Mr. Teh's learning of 'yab lek.' On the first day he responded with 'zum' in 3″. Somehow it seemed right to him, although it is an entirely new syllable. The second and third times he responded with 'rem' in 2.4″ and 13.6″. This is also a new syllable. He knew it was wrong when he repeated it but could not think of any other syllable that had an *e* in it. Then he found the association that the correct response begins with the letter before *m* and responded correctly the next time in 3.4″. On the second day this association had slipped away. 'Yab' suggested nothing the first time. The second time he thought of 'rem' again but he did not speak it because he knew it was wrong.

[1] The background for this paper is furnished by my two previous papers on Associative Aids.

The reaction times were 4.6″ and 10.6″ respectively. His old association now came back and the next time he responded correctly in 3.4″. On the fourth day, it failed to suggest 'lek' in 8″, but the second time the response was immediate, .8″. He responded correctly on the fifth and sixth days in 1.4 and 2.3″ respectively. The old association had dropped away, but 'rem' still kept popping into his mind.

In this case, the subject had to distinguish 'lek' from 'zum' and 'rem.' The latter was suggested by the thought that the correct response has an *e* in it, and this error was made a connecting link with 'lek' by the thought that the proper response begins with the letter before *m*. The thought of 'rem' was therefore the occasion not only for an analysis in that it had to be separated from the correct response but also the occasion for a synthesis of the correct response with its stimulus by means of a peculiar association. Although the thinking here is far from being syllogistic or logical it is nevertheless real and effective psychologically, and has the essential characteristics of thinking, namely, separating out and combining certain ideas in order to reach a desired end. The example given above involves more thinking than does learning a pair without association. In the latter, however, the response must be at least distinguished from nine others, and connected with a particular stimulus, although without any intermediate links. So long as this response must be made consciously and with attention, we should not hesitate to call it thinking, but with continued repetition the response becomes gradually unconscious and is made without attention. That is, it becomes a type of reflex action. Where this point is reached, there is no thinking in the psychological sense of the word, it is simply habit. This gives rise to the questions, What stage of the problem-solving process shall we call thinking? And what are the important characteristics of that stage? Before answering these questions, let us consider the influence of practice upon thinking (*a*) as it is expressed in association, and (*b*) as it is expressed in sensory content.

The effect of practice upon the course of association we have described in the previous paper. We noticed that they have a high frequency only during the early stages of learning. They are gradually eliminated through practice and do not occur at all in a mechanical response.

The effect of practice upon the conscious content of thinking was not within the purpose of our experiment since we asked our subjects to report only *what* they thought about between the stimulus and the response. A few of them, however, said that they saw such and such objects or printed words in their minds. The effect of practice upon these images was the same as that upon associative aids, *i. e.*, they appeared during the problematic stage of thinking and disappeared when the response became mechanical. If we examine the literature upon this matter, we find ample confirmation of this view. The experimental investigations of habit-formation and of the nature of thinking are studies in point.

The appearance of imagery and sensational elements in habit-formation was made an object of study by Book,[1] Rowe,[2] Angell and Coover[3] and others. Their studies are particularly valuable because their investigations were not based either for or against a sensational theory of thought. The following observations of sensory and imaginal factors in the development of voluntary control were made by Book, who studied the learning of typewriting: The subjects who learned by the touch method first learned the keyboard. One subject learned it so well that he could reproduce a map of it from memory. The keyboard was then screened and the process of writing began. In this the subject first got the copy; second, pronounced the letter; third, located it mentally on the keyboard, *e. g.*, formed a visual image of its position; fourth, got the finger on the proper key by first locating its row and then counting and feeling the individual keys until the proper one was found; and fifth, pronounced the letter again and made the proper movement. During

[1] 'Psychology of Skill,' 1908.

[2] *Amer. J. of Psychol.*, 1910, 21, 513–562.

[3] *Amer. J. of Psychol.*, 1907, 18, 327–340.

the early stages the letter was sometimes forgotten in this complicated process. In the course of improvement there was a gradual elimination of these numerous steps. The initial spelling of the words dropped away and the letters were correctly written by the sight of them. The mental locating of the keys and the complicated finger movements made in order to find the right key dropped away when the subject was able to find the proper key by the 'feel' of the movement necessary to hit it. The 'feel' also faded away when the subject was able to recognize the correctness of a key by the touch. The inner spelling continued the longest of these intermediate processes, but it also dropped away, so that the sight of the copy led directly to the hitting of the proper keys. The inner spelling, however, reappeared whenever difficult words were met.

Rowe, who had four subjects practice, until the reactions became automatic, writing ten standard words by pressing small rubber bulbs arranged so as to correspond in numbers and in letter-order to the lower row of keys in a Blickensderfer typewriter, reports the following about the appearance of imagery:

"It is only when a certain degree of skill and proficiency has been reached that imagery . . . of any sort becomes important. This imagery was uniformly derived from the preceding perceptual experiences in making the movements (voluntarily). . . . Much of the control imagery as such fades away with practice and . . . in the last stages of a voluntary movement a complex idea of the general situation, with little or no *particular* imagery of a clear character, is sufficient to carry on a series of practiced movements. In this stage the sensations arising from the movements themselves do not necessarily give rise to perceptions, but perform their proper functions without the aid of supplementary ideational processes."[1] Angell and Coover, whose experiment was referred to before; Leuba and Hyde[2] who had subjects practice writing English prose into German script; and Bryan and Harter,[3]

[1] *Amer. J. of Psychol.*, 1910, **21**, p. 536.

[2] Psychol. Rev., 1905, **12**, 351–369.

[3] Psychol. Rev., 1897, **4**, 27–53; 1899, **6**, 345–375.

whose subjects practiced on learning telegraphy, have made similar observations regarding the elimination by practice of sensory, imaginal, and associative processes intermediate between the stimulus and the response. Woodworth,[1] on the other hand, who sought to find the causes of a voluntary movement by having subjects do such highly mechanized acts as winking the eye, wagging the jaw, bending a finger, moving a forefinger to the nose, and opening the mouth, found very little imagery either immediately before the stimulus or between the stimulus and the response, and concludes that not any imagery is a determinant of voluntary movement. Against him are such experiments as those of Downey,[2] who had subjects do handwriting in new and difficult positions, and of Finkenbinder,[3] who had subjects solve difficult arithmetical and other problems, which find the employment of an abundance of imagery in the solution of their problems and, therefore, reach conclusions contrary to Woodworth's. But the explanation undoubtedly is that the experiments of Downey and of Finkenbinder come in the problematic stage of response, while those of Woodworth come in the mechanized stage. From these investigators who have observed the course of sensory and imaginal processes in habit-formations we believe that we may infer that their behavior is the same as that of the associative processes which we have made a special object of study. If we turn to the experimental studies of thinking, we find a number of reasons for taking a similar view as regards not only the sensory and imaginal components of consciousness but also the newly discovered non-sensory elements and forms.

The influence of practice upon sensory or imaginal components in the perception of letters is described by Clarke as follows:

"In the early part of the series, O. reports sensations from the different parts of the body, as the bending of the neck, stamping of the feet on the floor, pressing together of the teeth, and auditory images of the sound made by the finger

[1] 'Causes of Voluntary Movement,' Garman Memorial Volume, 351–392.

[2] Psychol. Monog., 1908, 9, No. 37.

[3] *Amer. J. of Psychol.*, 1914, 25, 32–81.

in moving over the paper. About the middle of the series he several times remarks: 'I did not notice the bodily adjustment of attention,' and in the last thirty or more experiments the introspection usually begins with the perception of the letter. If there is anything before this, it is usually the auditory sensation from the rubbing of the fingers over the paper. The strained condition of the body is mentioned only once in the last thirty cases. In the reports of F., also, we notice a marked falling-off in the number of conscious contents occurring before the appearance of the stimulus. The first introspection is almost entirely an account of the fore-period, and includes strain, breathing sensations, kinæsthetic, temperature, verbal and visual images, some of these occurring several times. In the experiments immediately following, the contents of this waiting period are only slightly decreased. Later the observer reports: Kinæsthetic and visual images of moving fast over the letter. Later, again: The 'ready' set me off without a conscious *Aufgabe*. The idea of movement with the tactual image is repeated a great many times, but toward the last is described as vague, and does not appear at all in some of the latest observations of the series."

That the *Bewusstheit* of Ach[1] appears only after a response becomes well practiced is indicated by the following observations as reported by him:

C.'s reactions to one of four letters, quadruple coördination:

"C., during the fore-period, had the necessity of repeating the coördinations. This imprinting occurred mostly in the manner that 'hb = right and dk = left' was spoken in inner speech. With this the proper voluntary sensations of the movement of the eyes or hand were imaged. After this had occurred in several experiments, there appeared one or two reactions the preadjustment of which was *unanschaulich*. . . . Afterwards an associative imprinting began again. Corresponding to the imprinting, the perception of the stimulus reproduced the image of the proper arm in the form of *Inten-*

[1] 'Über die Willenstätigkeit und das Denken,' 1905.

tionen, sensations of movement. The movement of the correct finger occurred immediately without any necessity of an image of the individual finger. In most cases the process took place in this way: the presented letter, *e. g.*, d, reproduced the image of d. The proper reaction resulted immediately from this middle term. . . . *Finally* . . . the clear apperception of the letter, which under these circumstances consumed the larger part of the reaction time, *selected after a short period, the proper reaction without the necessity of a middle term*."[1]

B.'s experience in reacting to the two numbers which he might add, subtract, multiply, or divide:

"During the course of the experiment, B. changed the condition of his 'set.' At first the goal idea appeared as an auditory kinæsthetic image, and about two or three times in succession. Since several premature reactions occurred during the first day and at the beginning of the second day, because of the familiar motor pressure to say 'pe' before the result was given, a precaution was added to the instruction already given, namely, not to react too quickly but above all to complete the reaction. Hence for several experiments the adjustment was prepared by saying, 'Add.' In consequence of the meaning of this operation, there was an optical plus sign in consciousness (in subtraction only a flighty minus sign in one case). This visual scheme disappeared after a few experiments to give place to a spatial adjustment of the attention in this way: the attention sought to embrace not one point but a large part of the shutter, and with this the momentary purpose was present as an awareness" (*Bewusstseinheit*).[2]

Another important concept invented by Ach is that of *Determinierenden Tendenzen*. The ground of this concept is found in such results as the following: When two numbers with a vertical line between them were shown, and the subject had the choice of adding, multiplying, subtracting, or dividing them, he, in the early stages of the experiment, usually in-

[1] *Op. cit.*, 143–144. Italics mine.

[2] *Op. cit.*, p. 175.

structed himself during the foreperiod by repeating his task in inner speech, or he saw images of mathematical signs between the numbers, or added them verbally, *e. g.*, 5 and 2 are 7. But in the later stages the result was immediate upon the appearance of the stimulus. It occurred without these images and without any awareness of what was to be done with the numbers. That is, there was an apperceptive fusion of the result with the stimulus. A similar experience occurred with the naming of alliterative syllables in response to nonsense syllables. Correct responses were made without any awareness of the task. The latter, however, came to consciousness when stimuli appeared that were not in agreement with the instruction.

The difficulty here as with Ach's *Aufgabe* is to explain the making of correct responses without being aware of it. But in the light of our knowledge of habit formation there is no excuse for the invention of new elements to explain these occurrences. The course of the responses here and of the accompanying consciousness present the usual characteristics of the process of habit formation. Making correct responses without being aware of it is the usual occurrence with perfected habits. The *Determinierenden Tendenzen* are merely habits.

There is also some evidence for interpreting the *Bewusstseinslagen* of Marbe,[1] Watt,[2] Messer,[3] and Bühler[4] as the conscious concomitants of highly practiced responses. As regards Marbe it is to be noticed that he observed them in correlation with responses to very simple problems, such as, What is the principal city in France? Translate 'Homo cogitat.' Are two and three six? How many are three times eight? To a graduate of a German university such problems could not provide much food for thought, and it is small wonder that most of the answers 'followed purely reflectively without further conscious processes' and that in many cases the subjects were conscious of nothing but the

[1] 'Experimentelle psychologische Untersuchungen über das Urteil,' 1901.

[2] *Arch. f. ges. Psychol.*, 1904, 4, 289–436.

[3] *Arch. f. ges. Psychol.*, 1906, 8, 1–224.

[4] *Arch. f. ges. Psychol.*, 1907, 9, 297–365.

sound of the question. Watt's experiment provided a better opportunity for observing the origin of the *Bewusstseinslagen*. His subjects during the early part of the experiment, in responding to stimuli under controlled association instructed themselves upon their task by repeating it in inner speech during the fore-period. Finally this process dropped away and the subjects became wholly unconscious of their tasks, but yet responded correctly to them in an objective way. In this stage the *Aufgabe* usually came to consciousness only in case of an error, *i. e.*, when the response did not agree with the *Aufgabe*, and a problematic condition again existed. Watt, therefore, has the problem of explaining a meaningful connection in a series of responses where the subject is not aware of it. Watt assumes that only some conscious factor can explain such an occurrence, and therefore makes the further assumption of an underlying *Bewusstseinslage* which directs the series of meaningful responses and which is in turn controlled by the *Aufgabe*. This would be a legitimate procedure if the explanation of conscious phenomena or of intelligent behavior would have to be restricted to the field of consciousness and if we were totally ignorant of the processes of reflex action and of habit formation in the nervous system. Since, however, unconscious action of the *Aufgabe* has all the marks of a mechanized response, why should we not be satisfied to call it a habit and explain it in terms of the known action of the nervous system rather than complicate the problem by the assumption of a *Ding an Sich* in the *Bewusstseinslage?* Watt, apparently, does not consider that a series of responses may have a meaningful connection objectively without its being so subjectively, that is, that a connection may be logical without being conscious. It is quite possible that a consciously meaningful connection may need the directive influence of a conscious *Aufgabe* without the same being true for an unconsciously meaningful or logical connection.

In this respect, Messer is truer to experience. He says that the consciousness of meaning may occur in various intensities 'from clear verbal images down to unanalyzable

Bewusstseinslagen.' The latter come in cases of automatic reactions and often after the reaction. This was the case in the meaning of *Maus* as a subordinate response to *Haustier.* He found that the consciousness of meaning as a distinct experience occúrs only in such situations as the following: when a word is (1) strange; (2) misread; (3) has many meanings; (4) is pronounced like other words of a different meaning; and (5) when there were two long words in the stimulus. These conditions are all problematic and easily lend support to the view that only undefined responses are accompanied by a vivid consciousness. Messer recognizes the same view when he discusses the difference between newly formed and reproductive judgments. He says that the mode of appearance and the degree of development in a judgment depend upon its newness. "The more that a reproductive character sets in, the more the activity-character of the judgment process is lost, the factors that above all give to the judgment—experience and expression—the conscious and intentional predicative relation, the seeking, considering, testing, deciding, admitting, and rejecting—now come less to consciousness as such or not at all. The process becomes smoother and continually more automatic. . . . In this way it can easily happen that an experience of an often-repeated judgment can no more be conceptually distinguished from a pure associative reproduction."[1] The implication that *Bewusstseinslagen* are the concomitants of practiced acts is also contained in Messer's discussion on the divisions between the formulated and unformulated thought. The division in question is not sharp. "On the one hand, the limits would appear to be a thinking in completely formulated sentences with a clear consciousness of meaning. On the other hand, a lightning-like reflection and recognition in which there is no trace of a word; and the difference must be greater than that between the slow and correct writing of a child who has learned to write well, and the flighty symbols of a practiced stenographer."[2] For an explanation, he states there is the

[1] *Op. cit.*, 125–126.
[2] *Op. cit.*, p. 186.

evident hypothesis that real psychic processes can go on abbreviated and telescoped in many ways and demanding more or less psycho-physical energy.

Bühler in discussing the thought experiences admits that a small group of them consist of images, either of things or of words. But the most important constituents are devoid of images and of every sensory quality. These are the *Bewusstseinslagen*. We take it that his *Gedanken* are a species of the latter. The implication that they are the concomitants of highly practiced responses is indicated by his definition of them as 'the last experiential units of our thought life.'[1]

Opposed to this interpretation is his view that conscious meanings are a species of *Gedanken* or *Wissen* without sensory components, a functional description of thought. He also gives many examples of *Bewusstseinlagen* which are complex and new thoughts. If this is true, we could not classify them as the concomitants of highly practiced responses, but would rather have to consider them as thoughts which had not yet reached a phenomenal formulation. This is the view expressed by Woodworth in his presidential address.[2] If we accept introspection as a legitimate method in psychology there is simply no ground for denying the correctness of this view. On the same ground there is no good reason why it should not be placed at both ends of the practice period. In any case, however, the preponderance of evidence is in favor of placing it near the final end of this period.

Besides *Bewusstseinslagen*, Messer discovered other contents of thought which he calls objective and conceptional thoughts, and spheres of meaning (*Bewusstseinssphären*). The difference between the first two consists in the presence and absence of imagery respectively. The spheres of meaning are simply directions of meaning. His examples of the first two indicate that they are mainly different stages of the practice period: Objective (Swan-duck. Second series, 995). "Dim image of a pond upon which something like a swan was swimming. I did not see a duck, but said 'duck' in an objective

[1] 'Tatsachen und Probleme zu einer Psychologie der Denkvorgänge,' p. 329.
[2] Psychol. Rev., 1915, 22, p. 1ff.

sense (with a direction outward, externalization)." Conceptional: (Swan-duck. Third series, 872.) "Entirely automatic without any particular after-thought. Pretty much tension. No visual images present." We would explain the qualitative differences in the contents in the two cases as a result of the repetition of the same stimulus rather than as a ground for two distinct types of thought. With a further repetition of the stimulus, there undoubtedly would appear still less consciousness and at a certain point meaning would be present only as a mere direction. At this point, we would also classify Bühler's *Regelbewusstseinen*, *Beziehungsbewusstseinen*, and *Intentionen* and as the last experiential conscious concomitant of a practiced response we put the *Bewusstseinslage*. Following this we would classify the response as habit and finally as reflex action.

Summing up the conclusions which we have drawn from the results and statements in the experimental literature on the thought processes, we would say that the influence of practice on the conscious contents of thought is similar to that on associative aids and on such conscious contents that came to our notice. The qualitative content of consciousness is largely dependent upon the degree of practice in the response which it accompanies. If we would arrange these contents in their genetic order as the response develops from an unlearned act to a habit, we should place them as follows: sensation, perception, imaginal thought, conceptual thought, awareness with direction, awareness without direction, habit, reflex action. The last two are unconscious. As regards the rest, we consider that the apex of conscious clearness is reached in conceptual thought. As regards the conscious presence of sensory components, we should consider them as ending in conceptual thought. We believe that this classification could be made to appear defensible, if it were worth while doing so, and that it also could be shown to be free from most of the contradictions which Rahn has pointed out to exist in the analytic, structural, and sensational theories of consciousness. But from the point of view of this experiment, it makes little difference what the conscious contents,

sensory or non-sensory, of thought are. We wish to discover how thinking does its work, where it begins, and where it ends. In this problem, an introspective analysis of the sensory or non-sensory components of thought could help us hardly a whit. At most they might be a symptom of the stage of practice, but of these there are so many objective marks which can be more easily observed. Again, we have no means of telling whether these sensory or non-sensory components are of any importance in the work of thought. At least we venture to believe that the experimental literature up to the present has not proven them to be so, we mean proof in the scientific sense of the word, a proof that is verifiable independently of the personal equation of the prover. We admit the necessity of a stimulus and of a response, but that processes consisting of sensory components having the attributes of intensity, quality, duration, and a possible admixture of clearness, and those other cobweb-like structures which have a textural difference from sensation in that they are 'more filmy, more transparent, more vaporous,' and are 'relatively pale, faded, washed out,' and 'misty'—must occur between the stimulus and the response and be carriers of meanings, this we do not admit. We fear that if the mind were stuffed with such things, they would stifle the work of thought to death, but we await the scientific proof of their function.

We must now revert to the original questions, How does thought do its work? Where does it begin and end? We would answer: Thought does its work by means of association. It begins with a problem or *Aufgabe* and ends with a solution. Thought is a form of controlled association, that is, the associations are directed toward an end. The data of our experiment lead us to this view. We noticed that the learning of the associated pairs was accomplished by means of associative aids in most of the cases, and, in the rest of them, the association was direct. Later, the responses became fixed and mechanized and the associative aids dropped away. The subject reported that they thought of nothing in these cases. The responses had become habits. Then when the words

were to be learned in new orders, backwards, downwards, and upwards, we find associative aids coming up again, and the responses indefinite and slow. A new problem had occurred and again the associative devices were set into operation to solve it. If these solutions had been repeated as in the regular tests, we assume that they would have followed the same course and would have become habits in a short time.

The experiments of Ach, Watt, Messer, Bühler and Koffka are not in disagreement with this view, although all of these investigators make a sharp distinction between thinking and association. Watt characterizes thinking as a meaningful sequence of ideas under the direction of the *Aufgabe.* In this it is different from association, which is a haphazard and undirected sequence of ideas. Under the *Aufgabe* there occur also many new sequences of ideas while an association is a mere restatement of an old experience. Ach takes the same view, and because in thinking there is a meaningful and often a new sequence of ideas, he must have determining tendencies to account for it, and therefore classes them along with association and perseveration as determinants of consciousness. Messer also finds various connections between ideas some of which are merely reproductive and accidental and some of which are consciously intentional. He limits the term thinking to the latter. Like Ach, Koffka[1] assumes the existence of determining tendencies to explain the solutions to *Aufgabe.* The task is often solved with difficulty, but an associative reproduction would be easy. There is selection in the ideas accepted as correct, and this selection cannot be explained by association. The ideas solving the task also are intentional and frequently appear in a nonsensory form. But neither of these characteristics is contained in the conception of association. We agree with Koffka that if the term association is to be restricted to the old conception, *e. g.*, to that of James Mill, we must assume the necessity of some such factor as a determining tendency for the explanation of the meaningful and new sequence of ideas found in thinking. But we see no reason for restricting ourselves to the old con-

[1] 'Analyse der Vorstellungen und ihrer Gesetze,' 1912.

ception. The synapses of the nervous system admit of millions of connections, and we assume the same to be true of our various experiences and thoughts. Under the guidance of instinct and interest there is no reason why there should not be a spontaneity in the connections between ideas. With that spontaniety come new and varied sequences. As a matter of experience these spontaneous connections in ideas are the usual occurrence. It is seldom that an old experience is reinstated with the exact order of the original. In fact, this rarely occurs without training and practice. The fact that repetitions are needed in memorizing is sufficient proof.

G. E. Müller[1] gives an extended criticism of determining tendencies, *Aufgabe*, etc., in his recent work on memory. He aims to show how the conception of association is sufficient for explaining the unconscious solutions to *Aufgabe*. We cannot reproduce his many important arguments, but we think that he restricts himself too much to the adequacy of the old conception and does not sufficiently recognize the importance of spontaneity and of habit.

Admitting the spontaneity of associative connections, enables us to appreciate the valuable contributions which Messer and Bühler have made to our understanding of the work of thought in getting the meaning of words and of sentences, reproducing associate phrases, analogous proverbs, and the sense of sentences from the keyword. These show quite clearly how thinking is essentially controlled association. It is one of the valuable results that has come from the discovery of alleged imageless thought. Having found that the introspective analysis of sensory components comes to grief and that the major part of our thinking goes on without them, these investigators struck out in a new direction and began to investigate the dynamics of thought. Messer in this connection discovered the importance of *Bewussteseinssphäre* in the suggestion of meaning. The meaning may come in the suggestion of the sphere of a superordinate, of a coördinate, or of the entire field of the object. It may also be suggested

[1] 'Zur Analyze der Gedächtnisthätigkeit und des Vorstellungsverlaufes,' III, *Zeit. f. Psychol.*, 1913, Erg.-Bd. 8, Sect. 126.

by an emotional state. Bühler did much more in this direction. He found that a sentence is usually understood by bringing it into relation with a more general principle from which it may be derived. But in some cases a sentence was understood by placing it in a schema, by recognizing its identity with a previous thought, or by translating it into a familiar form. The Analogy Tests brought out the associative character of thought more clearly. In these, E. read twenty familiar proverbs. These were followed by the reading of twenty other proverbs, each of which had a similar meaning to one in the first series. After hearing each one in the second series, the subject was to recall a similar proverb in the first series. In general the associative process was that the subject sought to comprehend the deeper meaning of the second proverb and while doing this a similar meaning in one of the first series occurred to him. Frequently the earlier thought was recalled through a generalized meaning of the thought and which was also valid for the earlier thought. The test thought also suggested the earlier one through a common single idea, a common word or phrase, a common whole meaning, a common sphere of meaning, or a common situation. All of these cases are good examples of association by similarity of meaning.

Although the members of the Külpe School seek to make a sharp distinction between thought and association, we interpret their results rather as showing that thinking is merely a species of association, that is, a controlled association.

We have noticed from the data of the experiment that these associative processes are employed in the solution of a problem only while it is problematic and that they drop away after the solution becomes fixed. We limit the term thinking to the associative process within the problematic stage and the fixed solution we classify as form of habit. In terms of formal logic the thinking would be called judging and the fixed solution a judgment. At least this is our psychological interpretation of these terms.

Summary V

The learning of paired associates may be treated as simple problems to be solved by thought. If so, we discover that associative aids have their highest frequencies during the problematic stages of thought and disappear as the solution becomes mechanical. This means that thinking does its work by means of association and when its work is done the associative process disappears.

Our observations indicate that the sensory contents of thought follow a similar course, an opinion which is amply confirmed by a review of the experimental literature on habit formation and on thinking. In the light of this view the numerous structural elements which the Külpe School has reported as existing in thought, such as Aufgaben, Determinierenden Tendenzen, Bewusstseinslagen, Gedanken, Regelbewusstseinen, Intentionen, etc., are quite unwarranted. So far as they exist they merely mark off different genetic stages in the development of thought from a problem to a habit. The qualitative content of thought is largely dependent upon the degree of practice in the response which it accompanies. We believe that these contents may be divided into the following stages of a genetic order: sensation, perception, imaginal thought, awareness with direction, awareness without direction, habit, reflex action. These distinctions, however, give us little help in understanding the real problems of thought: how it does its work, where it begins, and where it ends.

The distinction which the Külpe School draws between association and thinking is also unwarranted. The dynamics of thought may be fully explained by the concept of association. Thinking is a form of controlled association. It is merely a stage in habit-formation, beginning with a problem and ending with a habit. It does its work by means of associations and these disappear as the work approaches completion. Such a theory enables us to dispense with the fruitless quibbles over the multiplication of structural elements and makes possible practical studies of the dynamics of thought under a simple scientific concept which is in agree-

ment with modern physiology and scientific method. The discoveries of the Külpe School on the dynamics of thought instead of contradicting this theory really support it.

VI

Since both the methods and the conclusions of this experiment are at variance with the requirements of those who hold the sensationalistic theory of mind, we shall find it profitable to examine the differences and the justifications for them. As a representative of the sensationalistic theory I shall take Titchener and his school.

As a matter of method in this experiment I restricted my questions to demands for the reports of meanings thought of between the stimulus and the response. I never suggested that the subjects might have sensory or imaginal experiences in connection with these meanings nor did I ever ask questions which called for a report of these sensory experiences, if there were any. I therefore did not follow what Titchener calls the method of psychological description but instead the method which he calls the method of logic or of logical common sense. For brevity, I prefer to call it the method of objective report since the subjects report the objects of their thought, that is, their meanings or associations. I was tempted to do this partly because my subjects were not trained to report sensory experiences and partly because I was not inspired by the experimental studies which have been made by the method of psychological description. Bühler, in the course of his investigation, unconsciously drifted from the method of psychological description into the method of objective report. Titchener calls the latter method a failure because it commits the stimulus error. Yet I find that the observations reported by Bühler on sensations and images are the most worthless and unstable parts of his experiment. His important contributions to the future of psychology were obtained by the method of objective report. I refer in particular to his contributions to dynamics. If the studies made by the method of psychological description are lacking in valid judgments on dynamics, I attribute the results to

the method and not to the investigator. Let us examine by what method of reasoning the sensationalistic theory of thought is established. To avoid a lengthy discussion let us examine just one judgment that is frequently made by Titchener and the members of his school, namely that sensations and images are the carriers or constituents of meaning. I shall begin with Titchener.

"Meaning, psychologically, is always context."[1] "Meaning is originally kinæsthesis. . . . Afterwards, when differentiation has taken place, context may be mainly a matter of sensations of the special senses, or of images, or of kinæsthetic and other organic sensations, as the situation demands."[2] "As a matter of fact, meaning is carried by all sorts of sensational and imaginal processes."[3] "My 'feeling of but' has contained, ever since, a flashing picture of a bald crown, with a fringe of hair below, and a massive black shoulder, the whole passing swiftly down the visual field from northwest to southeast. I pick up such pictures very easily, in all departments of mind; and as I have told you, they may come to stand alone in consciousness as vehicles of meaning."[4]

Turning to Whipple's paper, 'An Analytic Study of the Memory Image and the Process of Judgment in the Discrimination of Clangs and Tones,' we find him reporting that Observer F.'s judgment is often conditioned by a loosening of the muscles of the scalp on the left side of the head or by a tightening of the muscles of the ear or by moving the head up and forwards, or by a pressure upward in the head. But he also makes many judgments without the presence of an image.

In Bagley's 'Apperception of the Spoken Sentence,'[5] we are told that no conscious 'stuff' was found which could not be classed as sensation or affection when reduced to the ultimate by a rigid analysis. The kinæsthetic elements are predominantly marginal elements and the marginal elements carry the meaning. "The apperception of auditory symbols involves the presence in consciousness of visual and verbal

[1] 'Text-Book,' p. 367.

[2] 'Exp. Psychol. of the Thought Processes,' p. 176.

[3] *Op. cit.*, p. 178.

[4] *Op. cit.*, p. 185.

[5] *Am. J. of Psychol.*, 1900, 12, 80–130.

ideas mainly; . . . the auditory and kinæsthetic elements seemingly form a small part, and the temperature, taste, and smell elements a still smaller part of this 'stuff.'" Some illustrations of the evidence upon which these statements are based are interesting..

"Not a man ha(s) had his vote refused him." L. visualized a voting card and a polling station. The vote was upon the 'license' question. The consciousness of this last reference took the form of the word 'temperance' printed with a capital T. There was some excitement about the city which in the ideal reproduction took the form of voice memories."[1]

"The safe door was closed with a sna(p) and the cashier was a helpless prisoner." With this sentence Wh. had a vague visualization of a man standing in the inside of a bank office. The safe door was back of him. He was a tall man with a smooth face and a derby hat. He had a notice in his hand and appeared to be startled at something; he was the cashier. (In this case, as in many other which we shall cite later, the reference was not in every way consistent with the context.)"[2]

"'He had lost ho(pe) in the unequal struggle.' Wh. felt sorry for the poor beggar."[3]

"'Fi(r)ing too high is a common mistake.' C. . . . noticed a general tension in trying to get a word for fi(r)ing—a strain about the eyes and chin."[4]

Helen Maud Clarke in her paper on 'Conscious Attitudes'[5] finds that they 'can be analyzed into sensations and images and feelings,'[6] and bases her conclusions on reports like the following:

The task of 'hurry up.' G. "Organic sensations from diaphragm. The muscles of the diaphragm seem to come up and press the lungs, and the muscles of the ribs seem to tighten."[7]

[1] *Op. cit.*, p. 108.
[2] *Op. cit.*, p. 109.
[3] *Op. cit.*, p. 124.
[4] *Op. cit.*, p. 125.
[5] *Am. J. of Psychol.*, 1911, 22, 214–249.
[6] *Op. cit.*, p. 249.
[7] *Op. cit.*, p. 231.

"Some of the reports show that the meaning of a word may be carried in whole or in part, by a motor image or an organic sensation: *Grip*. Visual image of a hand reached out to grasp something, and muscular image of the sensation in the right arm and hand when something is grasped."[1]

Probably the best examples of sensory meanings are given by Jacobson in his paper on 'Meaning and Understanding.'[2] A written word was laid before the subject for one minute. The last ten seconds were marked off by a signal, and the subject's task was to report what occurred in consciousness during this particular interval.

Obs. F., Stimulus, *heavily*. "An unclear auditory image of the noise. Strains in ear drum; organic sensations in the abdomen such as are involved in hearing a weight dropped, and such images as one would get from a jar of the building. . . . Tendency to nod head synchronously with the utterance: meant 'heavily.'"[3] The conclusion drawn is as follows: "The meanings of the stimulus words were thus carried by visual, auditory, and kinæsthetic processes; or to speak more precisely, the meanings which these processes bore were the meanings of the stimulus words, in so far as the latter were consciously realized."[4]

It may not seem just to criticize a method of which Titchener has said, after reviewing the experiments of Okabe, Clarke, Jacobson, and others, "A just appraisal will hardly give it rank with the approved methods of the science."[5] His objections are that the method in question cannot settle disputed questions of a systematic kind, or enable us to compose the issue between imageless and imaginal thought, or prove or disprove the existence of a 'form of combination'; and its results must admit of various interpretations and be subject to the charge of arbitrary selection. I agree with the objections and I should not say more, were it not for the fact that in the same paper he upholds Clarke's position that

[1] *Op. cit.*, p. 239.
[2] *Amer. J. of Psychol.*, 1911, 22, 553–557.
[3] *Op. cit.*, p. 566.
[4] *Op. cit.*, p. 564.
[5] The Method of Examination,' *Amer. J. of Psychol.*, 1913, 24, p. 440.

sensations and images are carriers of meaning. He upholds this position against Koffka's criticisms which were that sensory contents may be (1) irrevelant for thought; (2) a condition of thought, and (3) thought itself. Koffka finds fault with the author for recognizing only the third alternative to the neglect of the other two. Titchener replies that this criticism denies the possibility of psychological analysis in any field, *e. g.*, that the chord, c, e, g, could be analyzed into tones and noise.

I do not agree with this reply, but I insist that a judgment of a necessary connection in psychological phenomena must be grounded scientifically and logically in the same way as they are in any other science. It is one of the elementary lessons in logic that the occurrence of two events either simultaneously or in immediate succession is no proof that they are necessarily connected. Hume, to whom we are much indebted for the sensationalistic theory, has clearly shown this. I need scarcely recall J. S. Mill's five canons of experimental inquiry or the theory of probability in this connection, but the alleged connection between sensory components and meaning has not yet been verified by any of the approved canons of scientific judgment. Some sensationalists themselves admit the occurrence of imageless judgments and comparisons, *e. g.*, Bentley and Whipple. If the quality of the imagery is indifferent there ought to be at least some constant relationship between its amount and the amount of meaning or between the frequency of the imagery and the frequency of the meanings; but Clarke herself says that in thinking in verbal images 'it is by no means necessary to say every word which we should use if we were talking aloud.'[1] Even Titchener admits any quality or degree of imagery without a loss in the meaning, as in the recognition of a gray, and goes so far as to say that meaning may be carried in purely physiological terms.[2] The latter would be a meaning without sensations and images. If this is true, it is sufficient to disprove any necessary connection between sensory components and meaning.

[1] *Op. cit.*, p. 224.

[2] 'Ex. Psychol. of the Thought Processes,' p. 178.

Again, if the connection between meaning and sensations or images is necessary, those who investigate it by the same method ought to agree with one another. But of three men who are equally expert in introspection, none will reach the same conclusion. Jacobson, for example, asserts that sensations and images carry meanings. Watt and others of the Külpe School assert that there are imageless meanings, and Müller-Freienfels asserts that images are the *consequences* of thought and in no way the causes of the material of thinking.[1] The conclusion must therefore rest upon the personal equation of the investigator, which is another sufficient refutation of the alleged connection.

The occurrence of sensations and images may be due to many circumstances and may be independent of any connection with meaning except a temporal one. Titchener's image of a bald head when he thinks of 'but' may be a mere perseveration. The muscular movements of Observer F. which Whipple says condition F.'s judgments may be symptoms of indigestion only. When Bagley's observer Wh. sees a bank cashier with a startled look, a smooth face, a derby hat, and a valise in his hand, after hearing the sentence, 'the safe door was closed with a sna(p),' etc., the latter may simply have reminded him of a debt past due. When Clarke's Observer G. observes that the task of 'hurry up' consists of organic sensations from the diaphragm, or movement upward of the diaphragm, and a tightening of the muscles of the ribs, is it certain that the patient did not eat too many beans and too much bacon for breakfast? When Jacobson's Observer F. experiences various auditory, kinæsthetic, and other sensations as the meaning of the word 'heavily' after looking at it for fifty seconds, are not these experiences most naturally explained as a result of a changed direction of attention? A graduate student would not find much food for thought in looking at this word, and naturally he would turn his attention inward and observe the feelings of his organic processes. The latter would be all the more probable because of the instruction to get as much experience as possible in the

[1] *Zeit. f. Psychol.*, 1911–12, 60, p. 443.

last ten seconds. If some of these explanations are not scientific, is there not just as much logic in their favor as for the proposition that sensations and images are carriers of meaning? All that we ask is that such a proposition be proved in accordance with the approved canons of scientific judgment. In any case, we hope that our method of objective report has not proven less scientific or fruitful than the method of psychological description.

Summary VI

So far as the observers' reports are concerned, the method of this experiment was restricted to a report of the meanings or associations of thought between the stimulus and the response. It may be called the method of objective report, and as such is distinguished from introspection in its refined form of psychological description. The latter is based on the assumption that a causal connection in mental phenomena is a matter of immediate observation. This is illustrated in the evidence for the judgment that images carry meanings. Such a judgment ignores the most elementary lessons of logic and is not based on any approved canon of scientific judgment. In fact it is on a level with primitive thought, which takes an observed contiguity of two events as ground for a causal connection without a critical disproof of other possible explanations. In comparison to this the method of objective report when supplemented by a statistical treatment of its results is no less scientific. On the contrary we believe that it gives not only practical but also verifiable results.

COMMUNITY OF IDEAS

BY RUDOLF PINTNER

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The writer presents here some data relating to community of ideas. The experiment is the 'Class Experiment on Community of Ideas' prepared by E. G. Boring and G. M. Whipple and circulated among the members of the American Psychological Association some few years ago. The purpose of the experiment, according to Boring and Whipple, is "to demonstrate the existence of a high degree of likeness in the association of ideas of individuals when placed under like conditions and to measure the degree of this likeness in a number of instances."

Twenty stimulus words were used, the ten words of the experiment proper, and the additional ten words of what is called 'Supplementary Experiment *B*,' being words that are not supposed to show as much community as the first ten words. The following procedure, as described by Boring and Whipple, was followed: "See that the members of the class are provided with pencil and paper. Instruct them as follows: 'You will be asked to close your eyes and then to think of something that belongs to a general class, for instance, of any musical instrument, of any amusement, of any algebraic symbol. When the class of thing to be thought of is announced, notice what particular thing of that kind comes first to your mind. Do not search for anything else, but at once open your eyes and write the particular thing you thought of. Number the items you write to accord with the numbers of the trials as announced.' Give the following instructions and preface each with the command: 'Ready. Close your eyes. (1) Think of any color,' etc."

The observers have been divided into three classes: (1) University students, (2) school children aged thirteen and above, (3) school children aged twelve and below. The

university students numbered 739 for the first ten words and 567 for the last ten. There were 236 older children and 119 younger children. The data for the students were gathered during several years from several classes in elementary psychology. The first group consisted of 150 students and as each successive group was tested the results were added to the preceding results and the percentage frequencies were calculated. For the first ten words eleven groups were added to the original group, making twelve series of percentages for the occurrence of each association; for the second ten words nine additional groups were added, making ten series of percentages. A study of these percentages gives a measure of the stability of the frequency of each response. Long before the last group was added to the total these percentages on the whole showed little variation, an indication that the frequencies of the responses may be considered typical of students in psychology.

The data for the children were not treated in this manner because the numbers were not large enough. It is, of course, impossible to print all the successive percentages calculated for each response to each stimulus-word. The data would cover many printed pages. The general nature of the fluctuation of the percentages is obvious. During the addition of the first few increments the percentages tend to vary somewhat, while each new increment added tends to decrease this fluctuation. The average deviation of the percentages will be a measure of the stability of the percentage frequency. We have calculated this for the last six increments. This means the addition of 419 individuals in six increments. These average deviations for the three commonest responses are given.

Table I. gives the twenty stimulus words with their three commonest responses, the percentage of individuals making such response, the average percentage for the last six increments and the A.D. of these last six percentages. The small average deviations show very clearly the stability of the percentages for the commonest associations. If a class of university students in psychology is asked to 'think

TABLE I

THE THREE MOST FREQUENT RESPONSES—ADULTS

Stimulus	First Choice	Percent	Last Six Increments		Second Choice	Percent	Last Six Increments		Third Choice	Percent	Last Six Increments	
			Av. Percent	A.D.			Av. Percent	A.D.			Av. Percent	A.D.
1. Color	Red	61	64.1	2.0	Blue	26	24.9	1.3	Green	5	4.6	0.8
2. Furniture	Chair	80	81.2	2.0	Table	8	8.1	0.7	Bed	3	1.8	0.4
3. Flower	Rose	61	60.1	0.7	Violet	10	10.0	0.3	Pansy	6	6.2	0.2
4. Letter of the Alphabet	A	76	77.1	1.6	B	8	8.1	0.1	Z	2	2.2	0.2
5. Metal	Iron	46	47.0	1.3	Gold	29	29.9	0.5	Silver	5	5.5	0.3
6. Historic Personage	Washington	50	50.1	0.6	Napoleon	14	14.4	0.4	Lincoln	10	10.3	0.5
7. Part of Speech	Noun	46	49.0	2.7	Verb	36	35.0	1.2	Adjective	5	4.4	0.1
8. Geometrical Figure	Triangle	43	41.0	1.2	Square	23	25.4	1.7	Circle	11	11.0	0.3
9. Verb	Run	27	27.1	0.5	Go	24	23.7	0.3	Be	7	6.9	0.05
10. Tool	Hammer	46	45.3	1.3	Saw	14	11.8	1.2	Hatchet	8	8.7	0.4
11. Article of Food	Bread	52	51.6	0.6	Meat	7	6.8	0.3	Potatoes	6	6.8	1.5
12. Part of Body	Arm	35	33.0	1.3	Hand	23	24.3	1.4	Head	17	17.5	0.4
13. Day of Week	Monday	46	45.8	0.4	Sunday	20	20.0	0.6	Wednesday	10	8.7	0.8
14. Room in House	Parlor	28	28.6	1.2	Dining	18	16.0	1.7	Kitchen	13	13.3	0.7
15. Animal	Dog	37	34.8	2.3	Horse	27	27.1	0.8	Cat	16	16.0	0.6
16. Book	Psychology	12	13.9	1.1	Bible	6	8.8	1.9	Les Miserables	3	3.0	0.2
17. Girls' Name	Mary	20	29.0	1.0	Helen	9	9.0	0.6	Ruth	8	6.2	1.5
18. Source of Sound	Bell	15	18.2	2.7	Whistle	12	12.3	1.0	Piano	11	10.7	0.4
19. Date	Past	38	35.5	1.5	Recent	28	30.1	1.7	Future	16	14.5	2.3
20. Country	America	27	27.0	0.8	U. S.	20	19.7	0.5	Germany	16	16.0	0.7

of any color,' about sixty per cent. of the class are likely to think first of the color 'Red,' and so on with the other responses. The stability of some of the percentages is remarkable, as in the case of 'Flower—Rose' with an average percentage of 60.1 and an A.D. of 0.7; and 'Historic Personage—Washington' with an average percentage of 50.1 and an A.D. of 0.6. The highest percentage occurs with 'Furniture—Chair,' namely eighty per cent. The three commonest associations to 'Color,' namely 'Red,' 'Blue,' 'Green,' account together for ninety-two per cent. of all responses, leaving only eight per cent. of the responses to be distributed over all the other colors. In fact a study of the table impresses one with the great likeness in the association of ideas of individuals. The number of responses possible to the stimulus-word 'Verb' is very great, and yet the two responses 'Run' and 'Go' together account for over fifty per cent. of the responses.

Table II. shows the condensed results for the adults.

Table II

Adults

Stimulus	Word	Percent	Words Given Most Frequently								Failures, Percent
			By 25 % or Over		By 5 % or Over		By 2 % or Over		By Less Than 2 %		
			No. of Words	Total Percent	No. of Words	Total Percent	No. of Words	Total Percent	No. of Words	Total Percent	
1. Color	Red	61	2	87	3	92	5	96	6	4	0
2. Furniture	Chair	80	1	80	2	88	4	93	18	7	0
3. Flower	Rose	61	1	61	4	82	7	90	25	10	0
4. Letter of the Alphabet	A	76	1	76	2	84	4	88	20	12	0
5. Metal	Iron	46	2	75	3	80	8	96	9	4	0
6. Historic Personage	Washington	50	1	50	3	74	6	83	64	17	0
7. Part of Speech	Noun	46	2	82	3	87	6	95	12	5	0
8. Geometrical Figure	Triangle	43	1	43	3	77	9	92	17	8	0
9. Verb	Run	27	1	27	3	58	12	80	68	20	0
10. Tool	Hammer	46	1	46	5	80	8	88	29	12	0
11. Food	Bread	52	1	52	4	70	6	75	52	25	0
12. Part of Body	Arm	35	1	35	4	80	6	86	23	14	0
13. Day of the Week	Monday	46	1	46	5	92	7	98	0	0	2
14. Room in a House	Parlor	28	1	28	6	86	9	94	12	5	1
15. Animal	Dog	37	2	64	4	85	6	91	21	9	0
16. Book	Psychology	12	0	0	2	18	9	35	164	64	1
17. Girl's Name	Mary	20	0	0	3	37	15	64	86	35	1
18. Source of Sound	Bell	15	0	0	4	43	13	69	68	29	2
19. Date	Past	38	2	66	4	94	—	—	—	—	6
20. Country	America	27	1	27	6	82	9	89	24	10	1

The most frequent response with the percentage of frequency is shown, then the total number of words named by twenty-five per cent. or more of the individuals, then the total number named by five per cent. or more, and the total number named by two per cent. or more. The next to the last column shows the number and percentage of words named by less than two per cent. This is one measure of the amount of 'scattering' in the responses. The last column shows the percentage of failures to make any response to the stimulus word, or failure to follow the directions of the experiment.

A glance at the number of words given by twenty-five per cent. or over shows that no stimulus word called forth more that two such responses. Five words called forth two such responses, twelve words had only one response with a percentage of twenty-five per cent. or over, and in three cases the commonest response fell below twenty-five per cent.

The last ten words were supposed by Boring and Whipple to be likely to show less community than the first ten words.

TABLE III

OLDER CHILDREN

Stimulus	Word	Percent	Words Given Most Frequently								Failures, Percent
			By 25% or Over		By 5% or Over		By 2% or Over		By Less Than 2%		
			No. of Words	Total Percent	No. of Words	Total Percent	No. of Words	Total Percent	No. of Words	Total Percent	
1. Color	Red	46	2	77	5	94	7	99	4	1	0
2. Furniture	Chair	70	1	70	3	87	6	95	9	5	0
3. Flower	Rose	52	1	52	4	76	8	88	17	12	0
4. Letter of the Alphabet	A	69	1	69	2	77	8	93	18	7	0
5. Metal	Gold	41	2	66	5	85	9	96	4	1	3
6. Historic Personage	Washington	45	1	45	4	68	6	73	37	20	7
7. Part of Speech	Noun	54	2	80	2	80	5	89	6	3	8
8. Geometrical Figure	Triangle	26	1	26	4	61	4	61	14	9	30
9. Verb	Run	25	1	25	4	55	9	71	34	21	8
10. Tool	Hammer	53	1	53	3	78	9	91	13	8	1
11. Food	Bread	51	1	51	3	74	7	84	24	16	0
12. Part of Body	Arm	27	1	27	6	77	12	94	12	6	0
13. Day of the Week	Monday	48	1	48	4	91	7	100	—	—	0
14. Room in a House	Kitchen	24	0	0	5	91	7	96	7	4	0
15. Animal	Horse	40	2	66	4	83	7	92	13	8	0
16. Book	History	16	0	0	4	35	12	59	85	41	0
17. Girl's Name	Mary	17	0	0	2	23	15	58	70	42	0
18. Source of Sound	Piano	11	0	0	6	42	16	68	47	21	11
19. Date	Past	43	2	79	3	85	3	85	1	1	14
20. Country	America	24	0	0	5	82	9	93	10	6	1

For half of the words in question this seems to be the case. Numbers sixteen, seventeen and eighteen show the greatest amount of scattering. Number nineteen would show a great deal more if each separate date had been tabulated. Number eleven shows a fairly large amount of scattering in spite of the fact that the most frequent response has a high percentage. The other five words are not characterized by a great amount of scattering. There are three or four words among the first ten showing quite as much scattering as these five words.

Tables III. and IV. show the data for the children arranged in the same way as in Table II. for the adults. In general the characteristics of the children are the same as those of the adults, except that the percentages for the most frequent words are generally not quite so large, while the percentage of failures is much larger. With the younger children the percentage of failure amounts in some cases to over fifty per cent.

On the whole we may say that the more common responses

Table IV

Younger Children

Stimulus	Word	Percent	Words Given Most Frequently								Failures, Percent
			By 25% or Over		By 5% or Over		By 2% or Over		By Less than 2%		
			No. of Words	Total Percent	No. of Words	Total Percent	No. of Words	Total Percent	No. of Words	Total Percent	
1. Color	Red	49	2	78	4	90	6	96	3	1	3
2. Furniture	Chair	66	1	66	2	74	7	89	2	1	10
3. Flower	Rose	38	1	38	4	57	12	76	8	5	19
4. Letter of the Alphabet	A	37	1	37	2	44	10	64	7	13	23
5. Metal	Gold	35	1	35	3	50	6	60	1	1	39
6. Historic Personage	Washington	14	0	0	2	27	9	44	20	18	38
7. Part of Speech	Noun	18	0	0	2	34	3	36	4	3	61
8. Geometrical Figure	Square	16	0	0	3	32	4	35	1	1	64
9. Verb	Run	18	0	0	2	26	4	32	10	7	61
10. Tool	Hammer	52	1	52	3	74	5	80	5	4	16
11. Food	Bread	38	1	38	5	75	9	85	15	12	3
12. Part of Body	Head	18	0	0	6	59	14	80	8	5	15
13. Day of the Week	Monday	47	1	47	6	85	7	89	—	—	11
14. Room in a House	Bedroom	24	0	0	6	83	7	85	1	1	14
15. Animal	Horse	36	1	36	5	78	9	87	9	5	8
16. Book	History	15	0	0	3	30	12	58	31	27	15
17. Girl's Name	Mary	8	0	0	3	21	23	72	27	17	11
18. Source of Sound	Piano	10	0	0	5	35	17	65	18	12	23
19. Date	Recent	27	1	27	3	53	—	—	—	—	47
20. Country	Town or county	26	1	26	5	65	9	75	5	4	21

for the students are also the more common responses for the children, though the frequencies of occurrence often vary considerably. In general it is interesting to note that of all the associations possible to some stimulus words, how relatively few are chosen by two per cent. or more of the observers. Taking the stimulus words in order we note that 'Red' and 'Blue' are the two most common associations to 'Color' both for children and adults. For 'Furniture' the four most common associations for adults and children are 'Chair,' 'Table,' 'Bed,' and 'Desk.' There is a greater variety in the responses to 'Flower.' 'A Letter of the Alphabet' calls forth 'A' and 'B' as the two most common responses, but after that there is not much similarity. The younger children show a large percentage of failures. In response to 'Metal,' the three most common associations are 'Iron,' 'Gold,' and 'Silver' for both children and adults. 'Washington' leads in 'Historic Personages' and is followed by 'Napoleon' for the adults and by 'Columbus' for the children. 'Napoleon' does not occur in the lists for the children. The children's responses suggest the influence of recent history lessons. 'Verb' and 'Noun' are the most common 'Parts of Speech.' More than half of the younger children fail to understand this stimulus word. The adults show a large selection of geometrical figures, whereas a great many of the children fail to understand what is required.

'Run' is the most common response to 'Verb' for both children and adults, and it is difficult to explain why this should be the case. 'Run' is an extremely common word and is used in a great many senses, although in many cases not as a verb. The associations of a verb with doing something—an activity—may be a partial explanation. 'Hammer,' 'Saw,' and 'Hatchet' are the three most common tools for children and adults. There is not the great variation in response to 'Food' as might have been expected. The younger children differ somewhat in the most common response to 'A Part of the Body,' but the variation is not great. 'Monday' and then 'Sunday' show the largest percentages for 'Day of the Week.' The factor of primacy, first of a series, is no doubt the ex-

planation here, as it was in the case of the response to 'A Letter of the Alphabet.' The most common response to a 'Room in a House' is different for each of the three groups. 'Dog,' 'Horse' and 'Cat' are the three most common responses to an 'Animal' for both children and adults, although the children respond most frequently by 'Horse' and the adults by 'Dog.' It is interesting to note how very few of all the animals in the world occur among the responses.

When we come to the 'Name of a Book' we find a very great amount of scattering of responses. More than sixty per cent. of the adults give responses that occur very seldom. The response that occurs most frequently is the name of the psychology text that is used. The 'Bible' follows with a frequency of six per cent. 'Les Miserables' is the third most common response. There is no explanation to offer for this. The A.D. for 'Les Miserables' is 0.2, showing that its position is not due to any chance group of students who happened to be studying it at some particular time. The books mentioned by the children reflect strongly their school environment. There is a great variation in response to 'A Girl's Name,' although the most common response is the same both for children and adults. 'A Source of Sound' also shows relatively little community. In tabulating the response to 'Think of a Date, giving day of month, and year,' the responses were grouped under four headings, 'Past,' 'Recent,' 'Future' and 'Day of the Experiment.' If this had not been done the variety of responses would undoubtedly have been greater than the variety resulting from any other of the stimulus words. The response to 'Country' puts 'America' first for the older children and adults, but for the younger children we have a misunderstanding of the meaning of 'Country' resulting in their responding by giving the name of the town or county in which they live. We also note that six per cent. of these younger children respond by the name of a state. The relatively large percentage of responses received by 'Germany' is undoubtedly due to the fact that all these data were collected since 1914.

On the whole the striking points seem to be, (1) the narrow

range of variability in the responses, (2) the great similarity between children and adults, and (3) the stability of the frequency percentages of the commonest responses. The comparison with children would seem to indicate that for the greater number of these stimulus words the responses obtained from university students would not differ radically from those that might be obtained from the total population.

The percentages attached to each word may be used as the score for that word and in this way any subject's paper may be scored. The score will show the degree of 'community' expressed by all his reactions. Using the first ten words of the experiment we have calculated the scores for twenty adults and ten children each at ages fifteen, thirteen, eleven, nine, and seven. The percentage frequencies for adults were used in scoring all the papers. The total possible score is 536, *i. e.*, if the most common response were given for each of the ten words. None of the seventy subjects whose papers were scored reached this total. The nearest approach to it was a score of 473 made by an adult. The average and median scores for each of these groups are as follows:

Age	Adult	15	13	11	9	7
Average	387	380	373	320	187	146
Median	407	394	400	325	213	174
Range { Highest	473	454	472	416	323	294
Range { Lowest	162	281	271	171	26	13

There is a distinct decrease with age. Some of this is, of course, due to the inability of the younger child to comprehend instructions. None of the seven-year-olds reach the median of the eleven-year-olds. The range of scores in each group is very great. It would be interesting to study the relation of the degree of community of ideas, as measured in some such fashion, with the degree of general intelligence possessed by a subject.

PSYCHOANALYTIC CONCEPTS AND RE-EDUCATION

BY CARL RAHN

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It has become a commonplace to criticize the Freudian conceptions on the ground that they detain us in the realm of mythical abstractions instead of allowing us to pass on to the ascertainable relations that a descriptive and explanatory psychology sets out to discover. The underlying conception of a frustrated *Libido* that, Caliban-like, sulks and schemes in the depths of the unconscious, tends to obscure the concrete factual relationships between the psychological phenomenon and the physiological processes upon which it depends. This criticism is amply justified. . . . But insofar as the psychoanalysts obtain results in the practice of the art of healing and directly attribute these results to the alleged fact that Freud's terminology "penetrates causes . . . and provides a means of reëducation,"[1] the manner in which these concepts function in bringing about these results presents in itself a problem that the psychologist may study. His purpose may be either that of understanding the mechanisms of mind that underlie mental healing or that of further developing suitable methods for applying psychological principles to the art. It is the first of these purposes that motivates the following analysis of the rôle of the conception of the *Libido* in the practice of the psychoanalysts.

Four Factors in the Art of Mental Healing

Four factors that enter into the art of mental healing are (1) diagnosis of the disorder in terms of a clinical picture, (2) enthusiasm on the part both of physician and of patient, (3) a formulation of some sort of picture of the normal function-

[1] Jelliffe, S. E., *J. of Phil., Psychol., &c.*, 1917, 14, 270.

ing of the mechanisms involved, and (4) the release of the mechanisms that effect the reëducation.

Concerning the worth of the first of these factors little need be said here. The need for diagnosis is obvious. It is the first step in the scientific treatment of disease, but it is not the only step. Writing about the work being done today along the line of mental tests, Miss Martin[1] notes that nearly everywhere this work seems to end in the diagnosis, and that the therapeutic significance of what is learned from the diagnosis is too much ignored. Similarly, we find a vast literature in psychopathology of a diagnostic sort, that often invites the reader simply to share in the scientific delight over the discovery of a rare instance of some pathological phenomenon! And this is the case only too often in many of the discussions of the pathology of sex. To wade through these discussions is a tedious task—not so much on account of the pictures of human lives like lyres jangled and out of tune, but rather on account of the ever-recurring picture of lutes with one string on which no changes may be played. The picture of the normal may be illumined thereby,—but also, it may be obscured. Putnam writes that "few physicians read the works of v.Krafft-Ebing, Magnus Hirschfeld, Moll, and others of like sort."[2] It may be as Putnam believes on account of the taboo, but I recall the words once dropped by a successful and conscientious medical practitioner: "Psychopathology makes me tired. The less I know about the clinical picture, the better I am able to help the patient."

The evident appeal that Freud has for the healer, lies elsewhere than in the clinical pictures that he presents. He offers not only analyses and classifications of the abnormal, but in some subtle way he supplies enthusiasm and a program for action, or in the words of Jelliffe, 'a tool for again entering into facts and thus controlling them.' Herein lies, pragmatically, the virtue of Freud's system. Also this is why it is to be preferred, according to Jelliffe, to any contribution thus far made either by academic psychology or by behaviorism.

[1] Martin, L. J., *Science*, 1917, 44, 393–399.

[2] *Vide* Freud, S., Three Contributions to a Theory of Sex, New York, 1916, p. vii.

He tells us that Freud's terminology "penetrates causes and beginnings and provides a means of reëducation and redistribution of effect involving adequate discharge, before which the colorlessness and ineffectualness of an ideal behavioristic reëducation plainly reveal themselves. . . ."[1] Besides clinical pictures there "is that revivifying quality which preeminently distinguishes Freud's thought and which his followers have caught."[2] In fine, let us note that Freudianism apparently induces in Jelliffe and other healers a fervor and an enthusiasm which, if it infect the patient also, must go far toward reëstablishing a normal functioning in the psychophysical organism.

The third factor,—one that has never been adequately recognized by the critics,—is Freud's constant reference from the pathological to some sort of picture of the normal functioning of the mechanisms involved. We may, with Kraepelin,[3] look with concern upon the airing at any price of all possible sexual ideas both in education and in medicine. It may be the better part to respect the inhibitions that, according to Freud,[4] nature herself tends to build up in the developing personality. But whatever the wisdom of Freud's analytic procedure in actual practice, the fact remains that his whole method has led him at the same time to construct a picture of the kind of psychophysical organism the patient is to become; and in consequence he has developed, more completely than anyone else, out of the mass of concrete data on the human sex-life that his profession has enabled him to gather, a picture of normal ontogenetic development that represents a valuable contribution.

But these three factors alone—viz., analysis of the disorder, enthusiasm, and formulation of the ideal of behavior, —do not ensure a reëducation in the direction of the *realization* of the ideal as overt behavior. Experience has taught us that they may lead rather to mere sentimental day-dreaming alongside of which the old habits of muscular and gland-

[1] Jelliffe, S. E., *J. of Phil., Psychol., &c.*, 1917, 14, 270.

[2] Jelliffe, S. E., *op. cit.*, 272.

[3] Kraepelin, E., *Psychiatrie*, Leipsic, 1915, v.

[4] Freud, S., Three Contributions to a Theory of Sex, New York, 1916, p. 40.

ular reaction persist unchanged. There is a fourth requisite: the release of the mechanism of reëducation itself, a factor that accomplishes the transition from the undesirable to the desired form of behavior.

Functionally this fourth factor consists in a *redirection* of the neural excitation, not only in such a way as to obtain a new perceptual and ideational pattern, but also in such a way as to effect a propagation of the neural excitement over the appropriate efferent pathway. This involves in the first place not only a raising of the threshold for the old system of undesirable habit-arcs, but also a lowering of the threshold for the new system. And secondly, it involves not only attention to the new stimulus, the new ideal, but also a relaxation of that attention in order that the new response may occur. For the chief enemy of the new behavior is not necessarily the old established behavior-pattern, but it may consist in the process of prolonged attention to the new stimulus,—thus preventing the neural discharge to the glands and to the gross musculature of the body. Instead of overt action, we then have the consciousness of the stimulus. (This is a point that the behaviorists do not always adequately recognize.) Thus the 'ideal' may be said to harbor within itself the very mechanism that may frustrate its realization. It is therefore incumbent upon the healer to understand and to control the psychological mechanisms involved in reëducation, unless a kind Providence has already equipped him completely with the tact and the insight that make of him a natural healer.

The Process of Reëducation

When we inquire into the nature of the process of reëducation, we find that it involves two phases: first a new psychophysical set, a new patterning of the determining tendencies, must be created, and secondly the control of the mechanisms that release the new behavior when the stimulus is presented, must be insured.

Whatever may be the neural mechanism of voluntary action,—a moot point—it does seem to involve, physiologically, a preparation or readiness of the efferent path of the

arc so that at the moment when the refractory phase (?) supervenes in the act of attention to the stimulus, the excitation is readily propagated into and over an efferent path already primed and therefore of low resistance at all synaptic points from motor center to muscle or gland.

Psychologically, the burden of all latter-day discussions of voluntary action is that, contrary to the assumption involved in the principle of dynamogenesis, the 'idea' does not necessarily 'go over' into appropriate action. Ach,[1] in his attempts to state the facts of voluntary action in terms of principles, developed the conception of the 'determining tendency' which, in neural terms, involves the setting of the neural arcs in such a way that certain systems tend to be facilitated and certain other, antagonistic, ones to be inhibited. Broadening the conception, the determining tendency may be conceived either as an instinctive one, such as the hunger-set which the comparative psychologist may control in an animal for experimental purposes, or as an acquired one, such as the 'office set' of a business-man, under the influence of which the acts and ideas that are held in readiness will be very different from those which are in readiness when the 'at-home set' supervenes.

But besides this determining tendency that ensures the *preparation* of the paths into which the neural excitation may ultimately go, there must be, according to Ach, another factor that ensures the *release* of the appropriate act. In the behavior of the psychophysical organism the act of attending to the stimulus must be followed by the act prepared for,—else we have an organism lost in the contemplation of the stimulus. The factor that ensures this release of the specific act for which the organism has been prepared by the determining tendency, is, according to Ach, a mental attitude: "*Ich will wirklich.*"

Ach's account is based upon the analysis of highly conscious voluntary action, and this 'actual moment' of his, this "*Ich will wirklich,*" savors somewhat of the Jamesian *fiat;* yet the same factor is discovered also in the analysis of acts

[1] Ach, N., Ueber den Willensakt und das Temperament, Leipsic, 1910, 237–249.

that are not so definitely volitions of a highly conscious sort. In Miss Washburn's analysis[1] we come upon the 'activity attitude' which, as an essential factor in purposive action, corresponds to Ach's '*Ich will wirklich*,' but is not open to the same objections on the grounds of animistic implications. We thus note that both neurological and psychological considerations of the problem of conscious action must give an account of the two factors mentioned in an earlier paragraph: the preparation for the act, and the release of the act.

The Function of the *Libido concept* in Reëducation

These two factors, the one for obtaining the preparation, the 'set,' the other for obtaining the release of action, enter into all successful systems of reëducation. For obtaining the first, psychoanalysis has developed a bit of technique of decided pragmatic worth. If we may criticize the method at the very point where it usually prides itself on being particularly effective, namely in its practice of 'analysis,' which reveals to the patient his pathological condition,—then we must in justice point out also that its dangers are offset by the fact that, in the process of 'analyzing,' the picture of the normal emerges as the ideal toward which the reëducation is moving. This emphasis upon the picture of the normal operates psychologically in the same way as does the '*Aufgabe*' in the psychological laboratory. It sets the organism for the new behavior in the same way as the instruction in the laboratory creates a specific determining tendency in the subject.

The second factor entering into the process of reëducation is the mechanism for inducing the 'activity attitude' that favors the release of the desired mode of behavior. It appears that psychoanalysis possesses in its conception of the *Libido* an instrument admirably adapted for preparing and releasing the efferent mechanisms. The problem always is: how can this motor preparation and this activity attitude be induced, so that the individual will not merely 'know the good,' but will also do it?

[1] Washburn, M. F., Movement and Mental Imagery, Boston and New York, 1916, 161 ff.

The methods by which this is effected in education, in social control, and in healing, are various. The presence or the absence of certain objects or persons may do it; or again, some posture of the body; or again, some suggestive idea. The hypnotist does it by suggesting: "You will do as I say." My twenty-year old neighbor-boy clinches it by a dynamic formula, hand-illumined by candle-light; it reads: "Look to your *pep*." The psalmist affirms simply: "I shall fear no evil," and the inhibitions are released and he marches on.

Indeed the history of man's attempt to gain control over self is the history of the search for a formula that will give faith to act out a vision, to try the untried. And one of the oldest of the methods by which the readiness and release of the efferent mechanisms is ensured, is the use of the idea of a force, or energy, or power, surging and resurging through the organism, constantly seeking expression; and if blocked at one point 'seeking exit'[1] at another. It is the image clinched in the *Libido* of Freud and Jung, in the *Élan vital* of Bergson, in the *Wille zur Macht* of Nietzsche. Few ideas can compare with this one for tuning the organism for action. It is an idea to conjure with. No one, for instance, can come away from a reading of White's book on the 'Mechanisms of Character Formation' without a feeling of the actuality of the cosmic urge at the root of life.

The further development of the elements that enter into this psychoanalytic concept, that functions so effectively in inducing the 'activity attitude' is exquisitely illustrated in White. He goes beyond Freud and makes additions to the picture that add greatly to its pragmatic value. Not only does he see, with Freud, 'the will to power, as a great creative energy, streaming through the body,'[2] pouring through its channels, seeking exit in the form of some overt act,—but he sees it also as being transmitted from person to person in the social relationships in ever-widening circles.[3] And he would further enrich the concept by incorporating certain other dy-

[1] White, W. A., Mechanisms of Character Formation, New York, 1916, 323.

[2] *Op. cit.*, 258.

[3] *Op. cit.*, 333. "The symbol is the vehicle for the carrying of energy from person to person."

namic elements that make it resemble closely the central conception of ancient religious faiths. By way of Fabre's spiders that lived for seven months apparently without taking food, he arrives at the hypothesis that the animal organism can utilize solar energy directly, without fixation by chlorophyl, —and then advances the suggestion that 'the hundreds of thousands of receptors at the surface of the body' constitute 'a real and material source of energy which has been, largely at least, overlooked,'[1] Just how the cutaneous receptors are supposed to function in the novel capacity assigned them, White does not inform us; and it is not our purpose to enter into a critique of this curious conception of the sensory mechanism as an energy-intake. But let us note that White's conception of the sun as a source of energy that the organism can draw upon directly, through its sense organs, reveals striking kinship with the central conception of the once mighty sun-faiths of Asia and America, in which the orb of heaven was worshipped as the Giver of Life.

While it is wholly unjustifiable on scientific grounds to regard the body's receptor-equipment as an energy-intake paralleling in function the respiratory and the alimentary organs,—the hypothesis is nevertheless significant as indicating the type of 'dynamic' concept that psychoanalysis as a method of healing is developing. It is entirely in the direction of that conception that historically has proved itself of great pragmatic value in the religions of the race: the conception of a cosmic energy that flows in from a great universal reservoir, is transmuted and directed within the individual human being, and then flows out to bless the lives of his fellows.

Both in content and function the psychoanalytic concept is very like that of a contemporary Celtic[2] healer who, without claiming to be scientific, writes: "And so the holy Thing of Life passes unto the soul of the patient, and through that soul, it ultimates in the healing of the diseased body. Thus it is that, by merely opening the soul to receive the Divine

[1] *Op. cit.*, 241 f.

[2] Macbeth, J., The Brotherhood of Healers, Chicago.

influx, you allow the holy Thing to pass through you to another. . . . I am moved often to utter mentally, while healing, the word 'Love,' and to keep uttering it so long as the will to utter it is in me; and this seems to bring me into perfect harmony with the one source of Life—the Sun of Divine Love. For all healers are children of the Sun."

This is all of a piece with some of the variants of the Freudian conception of the *Libido*, so warmly defended by its protagonists. And it reveals the secret of its pragmatic justification: it induces in the healer and the healed an attitude highly favorable for the process of neuro-muscular and neuroglandular reëducation. For unless this requisite attitude is induced in the patient, no amount of analysis of abnormal action and thought, no amount of holding forth upon the normal as an ideal, will bring results. But if the psychoanalytic method has succeeded in creating, in the mind of the patient, an ideal of normal behavior as the goal of his efforts, which functions specifically in setting the psychophysical organism for the new behavior, in very much the same manner as does the '*Aufgabe*' or instruction in the psychological laboratory,—then the psychoanalyst's emphasis upon the idea of an effective psychic energy, the *Libido*, may be counted upon to create in the patient that faith or expectation, that 'activity attitude,' that psychology finds to be essential for the release of the specific overt response under the determining tendency created by the '*Aufgabe*.'

We must, however, distinguish between the content of the theoretical concept of the *Libido* in the systems of the psychoanalysts, and this its psychological function in the process of reëducation. We would call attention to its kinship with historical antecedents that have functioned similarly in man's practice of the art of healing throughout the ages. And finally we would point out that the psychological analysis of this function shows that the success of the psychoanalytic treatment, as of all other modes of psychic healing, is dependent in large measure upon the specific activity-attitude which it induces in the patient when he strengthens within himself the peculiarly effective belief that there is within and around

him an ever-present and never-failing supply of energy, upon which he can draw and thus re-create himself in body and in mind. In our modern world the naturalistic point of view has tended to crowd out the concepts of an earlier age when, through an Isaiah, the Ruler of the Universe invited the children of men to take hold of His strength, or when the psalmist petitioned: "Give Thy strength unto Thy servant,"—and forthwith the true believer was re-energized. Having crowded out these concepts with much flourish and noise, the contemporary world decks out other symbols in a pseudo-scientific terminology to serve the old, old need of keeping alive or re-awakening that faith that is the psychological pre-requisite for healthy action.

Conclusion

In the course of our analysis of the psychoanalytic method four factors were found to enter into the process of psychic healing: (1) analysis of the disorder, (2) enthusiasm, (3) the formation of the ideal of behavior, and (4) the creation of the attitude that will favor reëducation. The Freudians, to be sure, have tended to ascribe much virtue to the first of these factors; yet it is doubtful whether the picture of the abnormal, revealed in the process of 'analysis' to the patient, constitutes the most vital element in the method. But whatever the virtue of the analysis and of the revelation of his pathological condition to the patient, during which there is always the danger that some of the very safe-guards which nature herself, according to Freud, has erected in the course of the normal development, may be broken down,—the other three factors are of greatest import. Nothing limbers up the neuro-muscular and neuro-glandular mechanisms like enthusiasm. It lowers the resistance in the little used paths and it disturbs and loosens the fixed pattern of the old sensori-motor circuits of the pathological behavior. Again, holding up the picture of the normal—which may be a bit of technique that psychoanalysis has stumbled upon rather than consciously developed—performs in the process of reëducation the same function that the *Aufgabe* performs in the psychological laboratory.

And finally, in the conception of a vital energy that, by simple redirection, may become potent for good performance as it was for evil, the psychoanalyst has come upon a venerable and proven device for restoring the necessary faith that puts go and punch into the new behavior. If it frees the spirit from the fetters of fear and sin, from morbid gloating over abnormal symptoms and from sickly sentimental contemplation of the ideal of physical and moral health, and if it pulls the organism out of the rut of the old pathological behavior of nerve and muscle and gland,—if it does these things and so long as it does them, no concept, whether it be clinched in the term *Libido*, or *Wille zur Macht*, or *Élan vital*, or Divine Power, needs apology. It would be well, however, if both user and critic would distinguish between the scientific content of the concept and this its stimulus-value as a releaser of energy.

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NERVE PROCESS AND COGNITION

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The fundamental importance of the problems of psychophysiology would seem to warrant each sincere speculation concerning them. It is in this hope that the following argument is presented.

The attempt has been made to find a definition of cognition which naturally and of itself provides its own neurological explanation. Although the language of parallelism has been used throughout, this has been for convenience's sake only. I have not intended to prejudice arbitrarily the epistemological problem.

The definition of cognition which I offer is that it consists in a *placing of the given object or stimulus in some proper setting*. This was first suggested by a consideration of the question:—what would be objective (*i. e.*, behavioristic) evidence of cognition in another individual. Recent speculation[1] has advanced the proposition that the only sort of behavior on the part of a child which could be taken as proof positive of its consciousness of a given quality, for example, a color, would be behavior such as that exemplified in tests for color blindness. Professor de Laguna describes such behavior as involving "a *sorting* of the colored materials, an *arrangement* of them in a serial order. . . . Each tint or shade brings out a response appropriate to the specific character of the stimulus and characteristically different from the response demanded by every other tint or shade. The responses them-

[1] Grace A. de Laguna, *J. of Phil., Psychol., &c.*, 1916, 13, 533–547, 617–630.

selves form a series of graduated acts commensurate with the series of stimuli."[1] Such behavior involves, in short, a *placing* of the given quality with respect to others of its kind.

It will be my thesis that cognition always involves such a sorting or placing, but that the essential condition is an *internal neurological placing* rather than an overt objective one. For it is certain that one can and does cognize even when he is evincing no overt behavior. One knows himself to be aware of red on occasions when he is merely idly gazing at it and doing nothing objective with regard to it. This neurological placing, according to my theory, consists in an activity of association neurones which, under proper conditions, may lead to the objective sorting, but which can also go on without the latter.

Let us imagine a given colored light to cause a specific excitation in the visual areas of the cortex. This specific visual excitation we will assume tends to discharge into a specific association path. It will be the activity of this latter which, according to our theory, will constitute the sorting or placing, *i. e.*, the cognition of the given color. For we shall assume that the particular association path is what it is and derives its sole significance from the nature of its connections with other connected association paths. The path for red derives its significance from the nature of its connections with the paths for green and blue and yellow; while they in turn derive their character from the nature of their interconnections with one another and with the path for red. Similarly the specific path for the quality sour would derive its character from the nature of its connections with the paths for other taste qualities and so on.

To make such a hypothesis more cogent, let us attempt to work it out genetically in the case of an infant. We will suppose that, to begin with, the cortex of an infant is almost a *tabula rasa*. The stock of simple reflexes and instincts with which the child arrives into the world may be supposed for the most part to involve the lower centers only. Or, at any rate, we will assume that there is a large group of cortical

[1] *Op. cit.*, p. 545. (Italics mine.)

association neurones still undifferentiated. These undifferentiated cortical association neurones will offer the possibility of 'long circuit' connections between the same sensory and motor terminals more directly connected in subcortical regions by the reflex and instinctive equipment already on hand.

Now, let us imagine how specific paths may become developed in these hitherto undifferentiated 'long circuit' neurones. We will assume that, to begin with, every sense stimulus, in so far as its effect reaches the cortex at all, tends to dissipate its energies equally in all directions. All synaptic resistances are equal and there would be no tendency for any specificity of discharge in one direction more than in another. Consider, however, the effect which the repeated exercise of the reflex equipment may be supposed to have on such a condition. This reflex equipment contains characteristically different reactions for stimuli of different sense modes. Light causes eye movements, let us say, and pressure causes withdrawals of the part touched, and so on. Now, when one of these reflex acts is in progress, it means that any overflow of the stimulus which reaches the cortical levels will tend to go more directly toward the motor centers leading to that particular act than towards other motor centers. This follows upon an analogy with electrical circuits in parallel. And, as a result, different paths of low resistance through the association neurones will tend to be formed for the sensory centers corresponding to the different sense modes.

Consider now further the case of light. Imagine that a child is shown different colors, for example, red and yellow; and that when red is presented the nurse says 'red' and when yellow is presented, she says 'yellow.' We may suppose that the two colors produce different specific effects (spatial or chemical) in the visual area of the cortex. And we can assume, further, that there is a *reflex*, or perhaps *instinctive*, endowment whereby every sound has a tendency to arouse in the child an attempt at a vocal copy. Thus, accompanying the two different excitations in the visual area there will be (due to the activity of the lower centers) two different reflex

vocal accompaniments. As a consequence, we can expect that connected with each excitation a different specific path through the cortical association areas will tend to be formed. The visual processes corresponding to red will tend to become connected with the motor center for speaking the word red; the visual process for yellow with that for speaking the word yellow. We have already assumed, however, a single association path or group of association paths relatively open for all light stimuli: namely, paths leading to the motor centers for eye movements. We will now assume as an additional feature of our hypothesis that these new special groups of

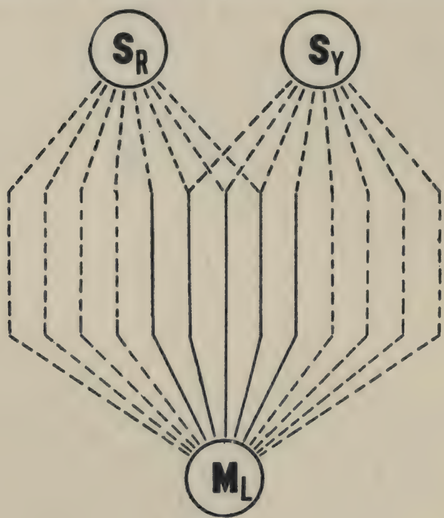

FIG. 1.

paths resulting from the speaking of 'red' and 'yellow' are influenced as to their direction by this already established group of general light paths. We can perhaps make this hypothesis clearer by means of diagrams (Figs. 1 and 2).

In Fig. 1, we represent the condition in which all light stimuli, of which red and yellow are examples, tend to go over into a single path (or rather group of paths), viz., that for eye movements. Thus S_r and S_y represent separate sensory cells (or separate chemical processes) corresponding to the

two colors. M_l represents the motor center or centers connected with eye movements. Both S_r and S_y can be conceived as connected with M_l by a very large number of possible paths (as shown by the solid lines and the dotted lines taken together). But however we envisage the difference between them, whether we think of S_r and S_y as different cells localized in different parts of the visual area, or as different physicochemical processes located within the same part, it may be assumed that some of the paths connecting S_r and S_y to M_l will be common (as shown by the solid lines). It is these common paths which will get developed as a result of the

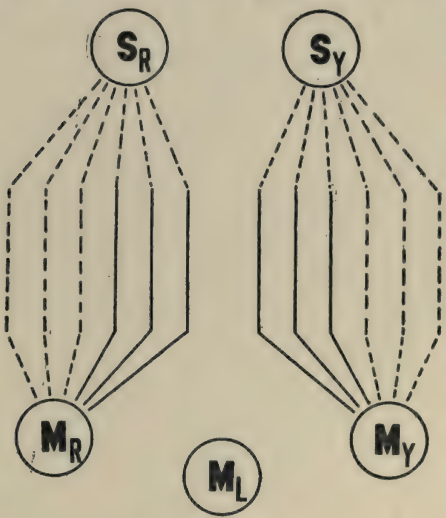

FIG. 2.

simple light stimuli and which will constitute the specific group of paths for light in general.

Suppose now that this common light path is achieved; how do the specific paths for red and yellow get developed? In Fig. 2 let M_r and M_y represent the motor centers connected with the vocalizations 'red' and 'yellow' respectively. S_r is connected with M_r by a great many different possible paths; and, similarly, S_y is connected with M_y by many different possible paths. Some of these in each case we may assume

would coincide for part of their length with the set of common paths used by both excitations in reaching M_l.[1] Since these latter are by hypothesis already well established, the resistance in them will be low. It seems reasonable to assume, therefore, that in the development of the new paths to M_r and M_y these old paths will to some extent be made use of; so that the final paths will take a course represented by the solid lines. As a result our final specific paths for red and yellow will be closely connected with the paths for light in general. (They will contain common elements with them.) They will also be closely connected with one another. In short, we will have the *beginning* of a system of paths, the activity of any one of which would constitute a *placing* of the object with respect to the system as a whole.

The objection may perhaps here be raised that the assumption of verbal instruction on the part of the nurse as a condition of learning to cognize the colors is unwarranted—that, in short, it is absurd to suppose that children must be taught to name colors before they can become aware of them. Now, as a matter of fact, I think it very probable that in a civilized community most children do learn colors as a result of being taught to name them. Still, if our theory is to hold water generally, it must allow for the savage or say the deaf mute, who learns colors without having words to express them. We shall lay it down as a general principle that, in such cases, the colors come to be discriminated only so far as they may serve as distinguishing marks for objects which otherwise tend to be confused. Suppose, for example, that a red sphere (apple) and a yellow sphere (ball) both originally cause the biting reaction in the child (as a result of reflex or instinctive endowment). Biting the ball, however, because of consequent discomfort, is followed by repulsion. Biting the apple, on the other hand, is followed by more biting. As a result, we may expect two different conditioned reflexes to be set up. Yellow sphere as stimulus will lead to something opposed to biting; red sphere as stimulus to reinforced biting. But the redness and the yellowness constitute,

[1] The paths going only to M_l are, for simplicity's sake, left out in Fig. 2.

by hypothesis, the only effective difference between the two stimuli. Here, therefore, just as in the previous example of repeating color names, we will have two different motor outlets set up for the two visual excitations of red and yellow. And we may suppose just as before that two different paths through the association areas will tend to be formed, one for red and one for yellow, and that the courses of these two paths will be influenced by an already established path for light in general.

A difference, however, between the end results is to be noted. In the case of learning the names, the 'red' and 'yellow' paths will finally terminate in motor centers for speaking red and yellow; in the present case, in motor centers for eating and repulsion. The final sets of paths would, therefore, hardly seem to be equivalent in the two cases. It is to be noted, however, that neither example claims to present a final picture. Before complete color perception can be achieved, many such occasions must occur. Red and yellow must be differentiated from one another in many different motor settings. They must also be presented with, and become similarly differentiated from, each and all of the other elementary colors. And as a result of each new occasion for differentiation new association paths will be formed. These new paths, however, will be influenced by, and will in turn influence, the already formed paths. Eventually, therefore, we may assume that only what might be called compromise paths will be left. There will be one group of these for each elementary color and every excitation corresponding to such a color will discharge for the first part of its course through such a compromise path, no matter what the motor center to which, under the conditions of the moment, it may ultimately lead.

This hypothesis resolves itself into three main points.

1. The constant and specific portion of each individual color path will be pushed back nearer to the sensory areas of the cortex and correspondingly further from the motor centers.

2. These constant and specific portions will simultaneously lose special connection with particular motor centers and will,

therefore, constitute compromise paths capable of leading equally readily to all motor centers.

3. The significance and the specificity of these compromise paths will consist in their relationships to one another and to the path for light, irrespective of color.

As a result of (3), the activity of any one such path will constitute the placing of the stimulus. If, for example, a given visual excitation discharges into the general light path, the stimulus will be seen (*i. e.*, placed) simply as light; if, on the other hand, it discharges into a specific color path, it will be seen (*i. e.*, placed) as a specific color. And similar systems of paths may be assumed to become developed for all the other types of sense qualities.

Before attempting to extend our hypothesis to objects other than sense qualities, let us here consider the theory as to the cognition of sense qualities developed by Bode.[1] The striking point of his theory is his definition of the function of consciousness as the *transference of the future into the present.* By way of illustration he uses, and emphasizes in particular, such qualities as 'sharp,' 'soft,' and the like. The "appearance . . . of a razor's edge as sharp is the sensory correlate of an incipient response which, if it were to attain full-blown perfection, would be the reaction to a cut. By hypothesis, however, the response is inhibited, and it is this inhibition which calls forth the perception of the object. If the response encountered no obstruction, adaptation would be complete and perception would not occur. Since there is a blocking of the response, nature resorts to a special device in order to overcome the difficulty, and this device consists in furnishing the organism with a new type of stimulus. The razor as perceived does not actually cut just now, but it bodies forth the quality 'will cut,' *i. e.*, the perceived attribute derives its character from what the object will, or may, do at a future time. That is, a perceived object is a stimulus which controls or directs the organism by results which have not yet occurred, but which will, or may, occur in the future

[1] Boyd H. Bode, 'Consciousness and Psychology' in *Creative Intelligence;* Henry Holt & Co., pp. 228–281.

. . . the future is transferred into the present so as to become effective in the guidance of behavior."[1]

The exact physiological mechanism by which this incipient motor response is supposed thus to body forth for consciousness a quality in the stimulus, is taken by Bode from Herrick,[2] one paragraph of whose discussion we may here requote:

"From the standpoint of the cerebral cortex considered as an essential part of the mechanism of higher conscious acts, every afferent stimulus, as we have seen, is to some extent affected by its passage through various subcortical association centers (*i. e.*, it carries a quale of central origin). But this same afferent impulse in its passage through the spinal cord and brain stem may, before reaching the cortex, discharge collateral impulses into the lower centers of reflex coördination, from which incipient (or even actually consummated) motor responses are discharged previous to the cortical reaction. These motor discharges may, through the 'back-stroke' action, in turn exert an influence upon the slower cortical reaction. Thus the lower reflex response may in a literal physiological sense act into the cortical stimulus complex and become an integral part of it."

Let us put this concretely into the case of Bode's razor. The razor, we are told, appears sharp because it calls out an inhibited subcortical tendency to react to it as to a cut. Let us think of this subcortical tendency as in the nature of a conditioned reflex.[3] We may suppose that in an earlier experience the razor actually did cut and that the reaction which instinctively or reflexly followed this cut (probably a dropping of the razor) became attached, because of the emotional intensity of the situation, to the razor itself as stimulus. This conditioned reflex, this dropping, is, however, by hypothesis inhibited; and it is the 'back-stroke' of this inhibited dropping which enters into the afferent impulse on its way to the cortex, and causes the quality 'sharp.'

[1] *Op. cit.*, p. 242.

[2] C. Judson Herrick, 'Some Reflections on the Origin and Significance of the Cerebral Cortex,' *Journal of Animal Behavior*, 1913, 3, 222–236.

[3] Bode does not himself suggest the term conditioned reflex, but I believe it to be what he means.

As thus stated, the theory seems striking and suggestive. Let us, however, examine it further. The conscious quality 'sharp' depends, we are told, upon two factors—an inhibited tendency to react as if to a cut, and the consequent 'back-stroke' effect going to the cortex. It is to be noted, however, that Bode does not fully explicate the function of the second of these two factors. The presence of the consciousness 'sharp' seems to depend primarily upon the presence and inhibition of the to-a-cut (dropping) reaction.[1] We are not told whether the going to the cortex of the 'back-stroke' of this inhibited reaction adds an essential or merely incidental factor to the production of consciousness. But if the addition of the cortical process is not itself an essential condition for the production of consciousness, then what is it which produces *conscious* awareness as opposed to a merely *unconscious* awareness? Bode does not explain how, as a result of frequent experience, one may come to treat a razor as sharp without having any *conscious* awareness of the sharpness as such. Obviously, however, such cases must be explained. If Bode's theory indicates any explanation for them at all, it would be that a direct subcortical effect of the inhibited dropping causes the *unconscious* treatment of the object as sharp, whereas the *cortical* effect of the inhibited dropping produces the *consciousness* 'sharp.' But, if such be the case, then our own theory is not so very far different. For in the instances of such qualities as 'sharp,' 'soft-appearing,' 'slippery-looking' and the like, we might readily admit that the 'back-stroke' from an inhibited motor tendency may constitute a necessary part of the afferent impulse coming to the cortex.[2] The essential thing is, however, that there would be no consciousness unless such 'back-stroke' does reach the cortex and hence by implication it is *something which happens in the cortex* which constitutes in the last analysis the ultimate source of consciousness.

Let us carry out our theory further in the case of 'sharp.'

[1] It is the inhibited to-a-cut reaction which 'transfers the future into the present.'

[2] We would not follow Bode, however, in extending such an assumption to all cases of cognition. It hardly seems a reasonable one in the cases of colors, tastes and other qualities which seem totally devoid of any inherent motor implications.

We would agree with Bode, and the functionalists in general, that consciousness usually arises in answer to a biological need. If, for instance, there were a tendency to treat the razor as both dull and sharp, then consciousness would be needed. At other times the shaving activity might very well go on without consciousness. A tendency to treat the razor as sharp means a conditioned reflex to drop the razor; while a tendency to treat it as dull means, let us say, a conditioned reflex to lay it down (originally the response to despair). Each of these two inhibited reflexes would affect the shaving activity in a different way, so that if both are present at once, there is trouble and the cortical activity is needed to settle between them.

Let us assume that the razor is really sharp. By hypothesis, however, the peculiar elements of the visual impulse, such as brightness and the like, connected with 'sharp' are not sufficiently strong for the appearance of the razor as a whole to produce subcortically the dropping tendency only. The laying down (dull) tendency is also excited, since the mere appearance of the razor in itself has become a major part of the stimulus for both dropping (sharp) and laying down (dull). A subcortical block from this conflict between the two tendencies results. The pure visual impulse reinforced by both 'back-strokes' goes up to the cortex. This is where the *logical* interrelationships of the cortical association paths prove their value. Logically a thing cannot be both sharp and dull, or, in other words, the interconnections of the 'sharp' and 'dull' association paths in the cortex must be assumed such that the activity of the one in like ratio inhibits that of the other. By hypothesis the purely visual part of the incoming impulse contained elements appropriate to sharpness only. But these will tend to excite the 'sharp' path and therefore to that extent to inhibit the 'dull' path. Hence the balance will be thrown on the side of the sharp. The 'back-stroke' of the inhibited laying down will not be equivalent to the 'back-stroke' of the inhibited dropping plus the purely visual elements. In the subcortical paths there was no such mutual exclusion of 'sharp' and 'dull' and hence

the mere sight of the razor could (before a nicety of association had been established) suggest both the sharp and dull reactions.

Once the cortical 'sharp' discharge is established, however, it tends (assuming the shaving activities still dominant) to go to motor centers which will reinforce the subcortical *effect* of the inhibited dropping; the subcortical block is broken and the razor is treated as sharp rather than as dull.

Finally, it should be noted that further experience with razors tends to make the initial tendency for a subcortical block less and less. The nicety of the subcortical associations becomes greater, so that the inhibited dropping reflex with its beneficial effect upon the shaving activities becomes more and more accurately attached to the visual excitation for sharp razors only, while the inhibited laying-down reflex becomes more and more attached to the visual excitation for dull razors only. When this state of affairs has been reached, there cease to be subcortical blocks, and, if the impulses happen to reach the cortex because of some chance low resistances, no particular advantage results therefrom. Consciousness in such a case is purely gratuitous.

Our divergence from Bode's theory may perhaps be summed up under the three following heads:

1. For many qualities, such as 'sharp,' 'soft-appearing,' 'slippery-looking,' and the like, the conception of a 'backstroke' as forming an essential part of the afferent impulse finally arriving at the cortex, seems a reasonable hypothesis. For other qualities, apparently devoid of inherent motor implications, such as colors, tastes, odors, etc., it seems absurd.

2. Even supposing cognition always *did* involve inhibited incipient motor tendencies, the agency of such inhibited tendencies would be required for the successful unconscious reaction just as much as for the successful conscious reaction. Hence the presence of some further differentia would have to be assumed. Such a differentia is provided, we would suggest, by *our* theory of the associative placing which occurs in the cortex.

3. And finally, we disagree with Bode in so far as he seems

to imply that conscious perception never arises except in answer to a subcortical block. We would suggest that it may arise as the result of no motor dilemma whatsoever, but simply because the cortical resistances happen at the moment to be low.

We may now attempt to extend our own theory to other objects of cognition besides sense qualities. Consciousness is more often concerned with complex objects, such as chair, finance, and the like, than it is with simple sense qualities. We would assert that the awareness of such complex and abstract objects is also due to associative placing.

The consciousness of chair, for example, may be supposed to be correlated with the excitation of a specific group of paths which have developed in the association neurones as the result of experiences with chairs of many different sorts and in many different connections. Just as the specific cortical paths for the individual colors were supposed to develop as the result of instinctively and reflexly doing things with regard to colors, so the specific cortical path for chair may be supposed to develop as the result of the instinctive and reflex things which tend to be done with respect to chairs. In the case of chair, however, the number of different appropriate motor situations will probably be less than in the case of colors. Therefore, the specific association paths which will finally get developed for the idea chair will be nearer to the motor centers and more closely connected with them than we supposed to be the case for the paths developed for colors. The significance of such chair paths will consist, in part, in their relations to coördinate paths developed for the other kinds of furniture (table, bed, etc.), and in part, on their relations to the paths for various complexes of sense qualities (such as four-leggedness, seatness, backness, etc.).

In the case of finance, the situation would be similar. The specific path for this idea would likewise depend upon its connections with other specific paths, for example those for commerce, industry, and the like, as well as those for complexes of sense qualities, such as the appearance of dollar signs, office-desks, and so on.

The consciousness of red, of chair, and of finance would thus, by our theory, all be similarly conditioned. Each would depend upon the activity of a specific association path (or group of paths), and the significance of each such path or group of paths would consist in its relations to other specific paths or groups of paths.

In this connection, an essential feature of the theory appears. The aspect of cognitive consciousness which is thus *immediately* correlated with the specific association discharge is, according to us, a *meaning*. Sensation or image aspects may be present also, but meaning (in the sense in which we are using the term) is the thing essentially present. We will not attempt to give an introspective account of meaning. We merely insist that in real life, if not in the psychological laboratory, the meaning is always present and often the only thing present.

Our doctrine here will best be clarified by expanding it in detail for each of the three types of object: sense quality, chair, finance. In the case of the first, i. e., sense quality, the meaning will be simple. It might possibly be epitomized in verbal terms: 'this which I know as blue' (or whatever the quality happens to be). Correlated with such meaning, and accompanying it, there will be in addition the *quale* of the quality. By the *quale* of a quality, as distinct from its simple meaning, I would indicate the *raw feel*, which is present in both sensing and imaging, but lacking in 'unanschauliches' thought. The meaning, as has already been posited, is conditioned by the activity of a specific group of association neurones. The quale, or *raw feel*, we will now posit as conditioned by the sensory neurones which empty into these specific association neurones. It is to be noted, however, that, according to this theory, the sensory excitation will not produce a 'raw feel' unless it goes over into an association discharge. Introspectively this would mean that the 'raw feel' of a quality never stands alone, but always requires its accompanying meaning. The corresponding genetic doctrine would, of course, be that the child, no matter how developed its sensory neurones, does not become conscious of sense stimuli, until

it has acquired meanings for them[1] (*i. e.*, learned to discriminate them).

So much for the sensing of qualities. Let us turn now to the remembering or thinking of them. The latter processes apparently may or may not include a 'raw feel.' There seem to be some individuals who habitually, and others who occasionally, think of, or remember, a sense quality without any conscious imagery. In such case, meaning alone is present. The explanation in these cases would be provided by the assumption that the specific association neurones have connections not only with their own specific sensory centers, but, as a result of associations formed by experience, with one another and with other sensory centers. Hence they may (in associative thinking) be excited independently of their own sensory centers. In these cases consciousness may contain merely the corresponding meaning divorced from any accompanying 'feel.'

Let us now explain the cases in which the memory or thought of a quality does involve an image or 'raw feel.' Professor Washburn[2] has suggested that "the nervous basis of the centrally excited sensation might be a discharge of the nervous energies stored up in a sensory center, induced by the excitation, from some other source, of a motor pathway into which that center had formerly discharged." If we substitute for the idea of 'a motor pathway excited from some other source' that of a *specific association path excited from some other source*, this suggestion will serve us. For then we may assume that an excitation of a sensory center may be induced by the excitation from some other source (as the result of associative connections) of the specific association path into which that center usually discharges. When this happens, consciousness will contain the 'raw feel' (*i. e.*, an image) in addition to the mere meaning of the quality.

Turn now, secondly, to a consideration of the more complex cognitive object, of which chair is an example. Here again two cases are to be distinguished: (1) sense perception

[1] See in this connection Professor de Laguna's article quoted above.

[2] 'Movement and Mental Imagery,' footnote p. 31. See also article by same author, PSYCHOL. REV., 1914, 21, 376–390.

(2) memory and thought. In the former, the specific association path corresponding to the meaning is aroused by the externally initiated sensory excitements of color, shape, kinæsthetic feel, and the like. Our theory posits that these excitements go over (for the most part) into the common chair discharge rather than into their own specific quality discharges. Therefore it is chair as chair, rather than a mere collocation of sense qualities, which is perceived.

It may perhaps be objected that, in perceiving a chair, one is conscious not merely of the meaning chair, but also of individual sense qualities, such as brownness, shininess, and the like. This must be admitted. When I perceive that chair yonder, I perceive it not only as chair but as brown chair. The brownness constitutes an essential part of my perception. Our theory must assume, therefore, that in such a case, the sensory excitement produced by the brownness of the stimulus empties not only into the general chair discharge, but also in part into its own specific brown discharge. In such a case, furthermore, since there is both sensory excitement and meaning discharge for the quality brown, the 'raw feel' as well as the meaning of brown will be part of the total consciousness. A similar argument would, of course, hold for other sense qualities which may make up part of the conscious percept.

Consider, now, the case in which the chair is not presented in sensation but merely remembered or thought of. The remembering or thinking of the chair depends, we may assume, upon the excitation of the chair path as a result of associative connections. But this path is usually touched off in sensation by a constant set of sensory activities such as those derived from the visual appearance of four-leggedness, the kinæsthetic feel of sitting, and so on. Such being the case, we may assume, just as we did for the simple sense qualities, that the associative excitation of the meaning path may arouse sympathetically those sensory centers usually emptying into it. If such a process happens and if further *these sympathetically aroused sensory centers then begin emptying also into their own specific quality discharges*, the subject will ex-

perience imagery in connection with his thought or memory. Different individuals may be supposed to differ in the extent to which their nervous systems are prone to this sympathetic arousal of the sensory centers and also, perhaps, in the extent to which such arousals then tend to go over into their specific quality discharges. The person in whom imagery is frequent would be one in whom such phenomena occur readily.

An interesting digression may here be made as regards the probable nature of introspection. The assumptions just preceding would suggest that introspection may involve primarily nothing more than the obtaining of specific quality discharges for sensory excitements which otherwise tend only to go over into more general discharges (such as the chair discharge in the above example). In the remembering or thinking of a chair, the chair meaning functions ordinarily for most individuals without *conscious* imagery. One has to be 'trained to introspect' before one finds much imagery, and then it is only by stopping and thinking back that one discovers it. It will be our contention that in such cases the imagery *really is not* present till one thinks back for it. The chair path or paths are excited; but it is not until one introspects that a sympathetic arousal of the sensory centers goes over into the specific quality discharges and one becomes aware of images.

The question may be raised how such a shunting of the sensory excitations into their simple meaning paths is accomplished. The answer would be that one has been taught to introspect; that is, one goes at his introspective task with various thoughts present as regards the kind of sensations and images he is to look for. In physiological terms, this means that there is a preliminary excitation of the simple quality paths. This lowers their synaptic resistance and sets them in readiness, so that *then* the sympathetically excited sensory centers more readily discharge into them.

But introspection is invoked not only for cases of memory and thought, but also for sense perception. Thus one might be set the task of introspecting one's visual perception of a chair. When not introspecting, one's perception of a chair

would seem often to consist of hardly more than the meanings chair, brownness, over there, etc., plus the 'raw feel' of the brownness and of the 'over-there-ness.' If one is asked to introspect, however, one becomes aware of many other sensory factors; various kinæsthetic feels, organic sensations of relaxation as in sitting, eye-strain, patches of white and black as well as of brown, auditory kinæsthesis for word, chair, and so on and so on. Till asked to introspect, these things were not present to consciousness. Our explanation is that the sensory excitations corresponding to these various sensations, which introspection brings to light, were, previous to introspection, simply emptying into and reinforcing the chairness, brownness, and over-there-ness discharges. The introspective attitude sets in readiness paths specific for these sensory qualities; whereupon the sensory excitements discharge into such paths and the qualities themselves enter consciousness.

Turning, finally, to the third type of object, the idea finance, little remains to be said. The situation is similar to that for chair. The difference is that in the case of finance the connections between the general meaning and specific sense qualities are more variable and dependent upon individual association than is true for chair. As in the case of the latter, however, the general meaning path may be set off by actual sensory excitations (in this case words) or as the result of associative thinking. And introspection will have the same task to perform in causing specific meaning discharges for sensory excitations which otherwise pass over into a general meaning discharge only.

Let us draw attention to the difference which our theory would posit between the consciousness of particulars and of universals. Meanings such as red, chair, and finance are universals. What is it then which constitutes the differentia of the consciousness of a particular red or of a particular chair? It might seem, at first, as if my perception of yonder chair as a particular depended, at least in part, upon the fact that it is a chair of a particular brownness, and hence upon the fact that in addition to the simple chair meaning I have the brown meaning. The particularity would, on this

hypothesis, be due to the fact of the presence of more than one universal meaning in combination. But a little further thought shows such a hypothesis to be inadequate. Yonder brown chair may be but one of a whole series of chairs all turned out by the same factory and all alike. Its particularity cannot, therefore, consist in its brownness combined with its chairness nor in any other mere concatenation of meanings. The one thing left to serve as the cause of its particularity is the fact of its yonderness, *i. e.*, the fact that it has a particular spatial and temporal meaning. A particular is a particular by virtue of its location in the temporal order or, in the case of a material thing, in both spatial and temporal orders. When, therefore, one is conscious of a particular, as opposed to a universal, our theory posits the presence of a temporal and perhaps also a spatial meaning in addition to other meanings.

Just how, in detail, the systems of association discharges corresponding to these spatial and temporal meanings are to be conceived would be a question which we will not here attempt to answer. It may be asserted, however, that whatever the details of the processes, they probably develop in ways analogous to that which we assumed for the development of the paths for the simple qualities, such as color. We may assume that different reflex and instinctive acts are performed as a result of different spatial and temporal relationships, just as they are performed as a result of different qualities or different concrete objects. We may suppose that cortical association paths develop in parallel with these subcortical reflex and instinctive processes. Furthermore, we would be inclined to assume that in the cases of these temporal and spatial objects Bode's hypothesis holds. We suggested that it probably did hold in the cases of such qualities as sharp, slippery-looking, and the like which seem to have motor implications. It seems, therefore, reasonable to assume that it holds also in the cases of spatial and temporal objects. For they certainly have motor implications. The whole accepted psychology of space perception has been concerned with indicating the importance of incipient motor responses

such as those of eye-movements, head-turnings, hand-reachings, and the like. The psychology of time perception is less developed; but, even there, the evidence points towards an importance of motor processes, such for example as those of breathing, beating time, and the like. We conclude, in short, that the effective afferent impulses arriving at the cortex which go over into the specific spatial and temporal meaning discharges probably contain as essential elements the 'back-stroke' action of inhibited subcortical processes.

In conclusion, we may sum up the significant features of our whole theory as follows:

1. Cognition consists in a placing of the given object or stimulus in a setting.

2. This placing consists neurologically in the *activity* of a specific path in the association neurones. Such a specific path owes its specificity to its interrelationships with other similarly specific paths for related objects.

3. The subjective aspect of cognition (*i. e.*, that which according to our theory is correlated with the activity of the specific association path) is a *meaning*. (No introspective account of the nature of meaning has been attempted.)

4. The essential cognitive experience is equally a meaning, whether the object cognized be a sense quality, a complex of sense qualities or an abstract idea. In the case of sense qualities, however, there may be in addition to the meaning a 'raw feel' (*i. e.*, if the quality is either actually presented to the sense organs or is present as an image). These 'raw feels' arise in connection with the activities of the sensory centers. They only appear, however, if there are corresponding association discharges, that is, if the corresponding meanings are also present.

EXPERIMENTS IN RATIONAL LEARNING

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Introductory

The study here reported stands midway between experiments on learning and intelligence tests. Most of the experiments that have been carried out on learning deal with practice effects that are in the main uninfluenced by rational behavior —such as typewriting, telegraphy, ball tossing, distribution of cards, segregation of wads, substitutions, discrimination of various sorts, memorizing nonsense syllables, and so on. Usually methods of procedure are rather clearly outlined to the subject in the instructions. Some of these learning processes are clearly concerned only with the acquirement of skill, little organization or conscious selection of any sort being required.[1]

Another type of learning has to do largely with the mastery of puzzles, problem boxes and mazes.[2] In some of these cases the situation remains unchanged in its essential features during the attempts at the solution of the problem; in others the situation to which the learner is reacting changes with the different reactions. In this general group of problems one successful solution may be sufficient to reveal the steps to be taken, so that subsequent efforts are practically perfect,

[1] E. g., E. L. Thorndike, 'The Psychology of Learning,' 1913. W. Brown, 'Habit and Interference in Sorting Cards,' *Univ. of California Pub. in Psychol.*, Vol. I, No. 4, 1914. H. Woodrow, 'Practice and Transference in Normal and Feeble-Minded Children,' *J. Educ. Psychol.*, 1917, **8**, 85–96 and 151–165. J. Peterson, 'Experiments in Ball-Tossing: The Significance of Learning Curves,' J. Exp. Psychol., 1917, **2**, 178–224. W. H. Batson, 'Acquisition of Skill,' Psychol. Monog. No. 91, 1917.

[2] E. g., E. H. Lindsley, 'A Study of Puzzles with Special Reference to the Psychology of Mental Adaptation,' *Amer. J. Psychol.*, 1897, **8**, 431-493. H. A. Ruger, 'The Psychology of Efficiency,' *Archiv. of Psychol.*, No. 15, 1910. J. H. Ballard, 'Some Phases of the Psychology of Puzzle Learning,' 1915(?). V. C. Hicks and H. A. Carr, 'Human Reactions in a Maze,' *J. Animal Behav.*, 1912, **2**, 98–125. F. A. C. Perrin, 'An Experimental and Introspective Study of the Human Learning Process in a Maze,' Psychol. Mon., No. 70, 1914.

or the relations may be so intricate as to require a series of successive trials for a perfect score. In mazes, for instance, the situation is often so baffling as to prevent at first the use of reason; the cues are so numerous that they cannot all be reacted to together or be held in mind by a person until after several repetitions have been made. A mental picture of the situation in its larger aspects may thus for a time be impossible, and the readiness of its establishment may depend to a considerable extent on the initiative of the learner.

Experiments on animals early aimed to incorporate some sort of more or less vaguely conceived rational or ideational element. Lloyd Morgan's idea of learning by trial and error answered the contention of Darwin, Romanes, et al. that animals reason, but a number of careful experiments were naturally to follow. Thorndike had animals react to various problem boxes, and found that the evidence was against the existence of reason and the use of ideas in such behavior.[1] The 'gradual slope' in the time curves he took to indicate 'the wearing smooth of a path in the brain, not the decision of a rational consciousness.' Hobhouse devised various tests which he contended required for their solution something more than mere association. Animals, he argued at length, showed 'practical judgment,' reactions based on the perception of relations among concrete objects, or on the perception of results affecting themselves. The experiments required reactions to several things simultaneously stimulating the animal, it was supposed, and they were devised to require some sort of conscious selection based on an anticipation of the results to be obtained. He admitted, however, that his results in the main were unfavorable to the existence in animals of ideas and of reflective imitation.[2] The multiple choice method of Yerkes, whose beginnings are to be found in Kinnaman's[3] and Hamilton's[4] works, was specifically designed, it

[1] These studies are now all published in book form, 'Animal Intelligence,' 1911.

[2] L. T. Hobhouse, 'Mind in Evolution,' 1901.

[3] A. J. Kinnamon, 'Mental Life of two *Macacus rhesus* Monkeys in Captivity,' *Amer. J. Psychol.*, 1902, 13, pp. 98–148 and 173–218.

[4] G. V. Hamilton, 'A Study of Trial and Error Reactions in Mammals,' *J. of Animal Behav.*, 1911, 1, 33 ff.; also 'A Study of Perseverance Reactions in Primates and Rodents, *Behav. Mon.*, No. 13, 1916.

appears, to present a situation that could not be solved by mere contiguity association. Certain results of this method have been interpreted by Yerkes as possibly showing ideation in the behavior of an orang-utan.[1]

A present tendency in psychology, not to be exactly identified with behaviorism, is putting the emphasis on mere contiguous association—after the order of the conditioned reflex of Pawlow and the associated- or psycho-reflex of von Bechterew—as possibly the only principle of learning, vividness and recency being used to supplement the frequency of the contiguity.[2] The operation of the vividness principle is, however, in most cases somewhat vaguely conceived. In fact, when we come to neural explanations it must be confessed that we are yet groping in ignorance. It is extremely difficult to conceive how elimination of useless movements is possible at all. The difficulties have been briefly indicated by the writer, among others, and he has shown that the maze learning of rats does not follow recency-frequency expectations at all, though these principles are not without influence, but that such learning *must at first, and actually does in the first trials by the animal, go contrary to these principles as at present understood.*[3] Further studies on humans by means of what is being called a mental maze are to follow.

The Problem

Our present study, though an outgrowth of these investigations, approaches the general problem of factors in learning from another angle. It was suggested to the writer in going over the recent studies of Hamilton and Yerkes, particularly in considering the significance of perseverance reactions (Ham-

[1] R. M. Yerkes, 'The Mental Life of Monkeys and Apes: A Study in Ideational Behavior,' *Behav. Mon.* No. 12, 1916. References are given to other studies by the multiple choice method.

[2] This tendency is, of course, not a new one in psychology, but it is based more definitely on experimental evidence. Though the experiments themselves are somewhat limited in their direct application, it is tempting to extend theoretically their significance.

[3] 'Completeness of Response as an Explanation Principle in Learning,' Psychol. Rev., 1916, 23, 153-162; 'Frequency and Recency Factors in Maze Learning by White Rats,' *J. Animal Behav.*, 1917, 7, 338–364. Other references there given.

ilton). A sort of choice-reaction problem is presented to the human subject, about whose possession of relevant ideas and of rational ability there is no doubt. It is our special purpose to see how rational learning is related to learning that must depend wholly on 'trial and error' efforts. How effectively are ideas used in a type of learning in which their employment is obviously helpful?[1] The reaction required of the subject is to associate in a random order the numbers 1 to 10, inclusive, with the first ten letters of the alphabet. This is to be done by means of a series of guesses the range of which may be greatly limited by the use of a rational organization of the situation. Each subject completes the learning at a single practice period, varying in length inversely with the subject's ability, roughly speaking. As will be seen, the subject is forced to react to a changing situation, each response making it different to a slight degree by limiting the range of probability.

The Method

The experimenter in giving the 'tests,' as they were called in talking about them to the subjects, sat at a table opposite the subject shielded from the view of the latter by a screen. The experiments were carried out in a quiet room by the writer. The following instructions were given each subject:

"This is a memory-reason test. The letters *A*, *B*, *C*, *D*, *E*, *F*, *G*, *H*, *I*, and *J* are numbered in a random order from 1 to 10. I call out the letters in their order and you are to guess numbers for each letter till you get the correct number, when I say 'Right.' Then I call out the next letter, and so on. This is continued till you get each number right the first guess twice in succession through the series, from *A* to *J*. Then you are through. *You must ask no questions*, but are to use all the mental powers at your command. You will be judged by the number of errors (or wrong guesses) you make and the number of repetitions from *A* to *J* required."

The subject was left free to use any method in establishing the associations, that occurred to him. Nothing was said

[1] For an objective conception of 'idea' see an article by the writer in Psychol. Rev., 1918, 25, pp. 214–226.

regarding introspection, though some of the subjects volunteered information of an introspective nature after the experiment was completed, and others were questioned about certain irregularities noted in their behavior and about matters on the interpretation of which the experimenter desired more light. Such questions were in every case asked immediately after the completion of the experiment. Specific cases will be given later. The lack of systematic introspection is justified by the fact that a rather complete record of the subject's reactions was made, and also by the desirability of having no disturbing factors enter into the learning process. The objective record was in many cases found to be much more reliable than the subject's memory, regarding the clearness with which certain elements had been held in mind.

All the subject's relevant reactions were accurately and easily recorded. The method is objectively shown in Table I. The letter *A* was first called out and each of the subject's guesses was recorded, in order, under *A*. Hesitancy or extreme deliberation was usually indicated by a certain mark. At first the subject would usually guess only one number and then wait till he was told to proceed as instructed. When the correct number, 9, was guessed the experimenter would say *right!* and call out the next letter, *B*, and so on. When all letters to *J*, inclusive, had been completed, *A* was immediately called again without any interruption. After the first series each successive letter was usually called as soon as the correct number of the one preceding it was given, and the response *right!* by the experimenter, now unnecessary, was omitted.

The subject not infrequently guessed a number and quickly corrected it; as, "6; no, 7," or "6; no, I guessed that for *B;* 8." In all such cases the number thus clearly spoken and retracted was, however, recorded as a guess. It often happened that the subject would begin saying some number, as 'sev—' for seven, and not complete it. A reaction of this kind was not recorded as a guess although it was frequently evident which number was in mind. Such reactions were, however, indicated, though not with entire regularity in the beginning of the experiment. It was later noticed

that they might have considerable significance and throw light on the nature of the learning. In regard to this type of behavior different subjects showed marked differences; some would think the whole process out before speaking the number, while others were inclined to do more or less of their thinking aloud. A number of them employed their fingers in various ways, while others, as they explained later, attempted to associate the forms of the related figures and letters. No particular method seems to have universal advantage, or to be related to marked success or lack of success, though this matter may well be left open for special study.

The Subjects

The subjects are described individually on a later page. All were adults but one. Special training in the observation of one's own mental processes was not desired in this case, the purpose being only to get persons of ordinary rational capacity. Some of the subjects had had a number of courses in psychology.

Results

The results of each individual have been tabulated for special study. The presentation and analysis of a few typical cases is essential here, even at the expense of a discussion of the relation of rational to other learning. Table I is the record of a junior college man of fair mental ability, probably slightly above the average in his section.

In this record every reaction is shown in its order of occurrence. It is interesting to note that the greatest number of errors is made with the letters at the middle of the series, *i. e.*, with *D*, *E*, and *F*. But for an obvious confusion in connection with *I* and *J* in the 5th, 6th and 8th series, there would have been an almost symmetrical sloping in the total unclassified errors at the base of the table, toward the *A*- and the *J*-end of the series. The first letters of the series seem to have a much better memory-value position than the later ones, despite the fact that in the early repetitions through the series the possible minimizing of errors through a logical limitation of the number to be guessed was gradually increased toward *J*. In

TABLE I

RECORD OF MR. A. B., JUNIOR COLLEGE MAN

Letters Numbers	*A* 9	*B* 6	*C* 2	*D* 10	*E* 8	*F* 1	*G* 5	*H* 4	*I* 7	*J* 3	Errors Unclassified	 x	 *	 Total
First series (or trials)	3 4 5 6 9	4 6	5 7 3 1 2	7 10	4 2x 1 6x 9x 7 3 5 4 10x 8	4 3 7 10x 5 3* 1	3 7 4 5	1x 7 3 6x 7* 4	1x 7	1x 3	36	9	2	47
Second series	9	4 2 3 6	2	5 4 7 9x 10	7 4 1 5 2x 6x 7* 8	5 6x 3 1	9x 10x 7 4 3 5	3 2x 1x 9x 6x 5x 8x 9x* 4	10x 3 7	3	32	14	2	48
Third series	9	5 7 2 4 6	2	5 8 9x 4 1 6x 9x* 7 3 5* 8* 10	7 5 2x 4 6x 1 3 5* 8	5 2x 4 9x 7 6x 5* 4* 3 1	6x 2x 4 8 9x 7 5	3 7 4	3 5x 6x 7	3	43	13	6	62
Fourth series	9	6	2	5 7 4 6x 1 3 10	4 1 7 8	4 3 1	7 5	4	3 7	4x 3	14	2	0	16
Fifth series	9	6	2	10	8	1	5	4	1x 5x 9x 7	3	3	3	0	6
Sixth series	9	6	2	10	8	1	5	4	7	1x 6x 9x 7x 5x 4x 3	6	6	0	12

TABLE I—*Continued*

											Errors			
Letters .... Numbers	*A* 9	*B* 6	*C* 2	*D* 10	*E* 8	*F* 1	*G* 5	*H* 4	*I* 7	*J* 3	Unclassified	x	*	Total
Seventh series	9	6	2	10	8	1	5	4	7	3	0	0	0	0
Eighth series	9	6	2	10	8	1	5	4	3 9x 2x 7	2x 9x 4x 6x 8x 9x* 3	9	8	1	18
Ninth series	9	6	2	10	8	1	5	4	7	3	0	0	0	0
Tenth series	9	6	8 10 2	10	6x 1 2x 8	1	5	4	7	3	5	2	0	7
Eleventh and twelfth series correct														
Total U.C. Errors	4	8	6	22	31	20	15	15	13	14	148			
x Errors	0	0	0	5	10	5	5	9	9	14		57		
* Errors	0	0	0	3	2	3	0	2	0	1			11	
Totals	4	8	6	30	43	28	20	26	22	29				216

other words, the guessing of *A* in the first series was by pure chance, its probability of correctness in the first trial being 1 to 9; that of *B* (if strict logic and perfect memory were employed) was limited by the subtraction of the used number, 9, and would be 1 to 8; that of *C*, 1 to 7; and the probabilities of a correct guess the first time under these conditions would be 1 to 6, 1 to 5, 1 to 4, 1 to 3, 1 to 2, 1 to 1, and 1 to 0, respectively for *D*, *E*, *F*, *G*, *H*, *I*, and *J*. With perfect logic and memory *J* would always be correct, and all guesses in subsequent series would always be correct. Our subject, A.B., falls far short of such perfection! He was definitely warned that "this is a memory-reason test," and was advised to use all the mental powers at his command. He continued to make frequent errors, even in the 5th, 6th, and 8th series on the letters *I* and *J*, when every error made consisted in naming

for these letters numbers which he *knew* belonged to other letters and which could therefore not be right for these cases. This statement he himself admitted, after the experiment was completed. The errors were made because under the conditions he 'didn't think of it.' Such a reaction proved to be by no means peculiar to this subject, showing that in the stress of a difficulty, not at all different from many that one must constantly meet in practical life, one's reactions may become limited to a suprisingly narrow aspect of the situation, or, in subjective terms, consciousness becomes thus limited.

We have classified the errors into three kinds: (1) unclassified, (2) logical, (3) and perseverative. The first needs no further explanation. The *logical errors* are those indicated in the table by an x, and consist in guessing a number that has already been used for an earlier letter of the series, one that could therefore not possibly be correct. It is, of course, true that not only bad logic but bad memory is responsible for such an error; in fact, the error may occur, and frequently does occur, when the subject would know perfectly well on a little thought that it is wrong, as has already been indicated. In one sense, as we have pointed out, all errors in the second and subsequent series are logical errors as the term is here defined; but we have chosen to make the arbitrary limitation here described, because the subject has a decided sense of getting back to the starting place when the first series is completed. *Perseverative errors* are errors of repeating a wrong guess while reacting to a single letter. In Table I such errors have been indicated by an asterisk (*). Since this is a study of rational learning, and such errors are more serious than those made without the possibility of logical inference, it has seemed desirable to count an extra point against the subject for each logical or perseverative error, and two extra points if any guess is both a logical and a perseverative error. The errors for *I* and *J* in the 5th, 6th and 8th series, for example, are more serious than those for *A*, *B*, *C*, and *D* in the first series—in this case many times more serious, perhaps, but we have had to adopt some sort of practicable classification for errors of different kinds. Such arbitrary limita-

tions and distinctions must always be given under circumstances similar to these, and this is one source of fallibility in all intelligence tests.

It is obvious that our subject has made a number of logical and perseverative errors that are due to thoughtlessness and that could have been avoided with sufficient care. Often, as we might expect, such errors were due to emotional excitement under competitive conditions, each student being anxious to stand comparatively high in the tests. In many cases they were clearly due to too much attention to one particular aspect of the situation to the neglect of other relations. Differences in this respect seemed to be particularly due to (possibly innate) differences in mentality, a matter to be considered in later pages.

Two other records, which it seems worth the space to consider separately, will show different methods of such limitation of attention, while one record that is almost logically perfect will illustrate the advantage of reacting to a wider range of the essential or conditioning factors.

Table II gives the record of Mr. L. D., a sophomore student who ranked in work in the psychology class with the best fourth of the students. This subject failed to react to all the significant factors in the situation because he was, in a sort of random way, looking for some regular order in numbering. At first he reacted so slowly that occasionally he would sit in a sort of stupor between each response. As a consequence of this he often repeated a number (perseverative reaction) because he had forgotten what numbers he had already guessed for the letter in question.[1] He suddenly made great improvement, however, when he gave up the futile search for some principle in the numbering. After the experiment he reported as follows: "I hadn't at first formulated any plan, but tried to remember without any organization; was alert for any scheme or regular design in the numbering. Then I tried to remember the correct numbers in their true order of sequence, with little attention to the

[1] To prevent this condition the instructions to subjects in later experiments, subjects not included in the present group, name time as one of the criteria of efficiency.

Table II

Record of Mr. L. D., a Sophomore Student

Letters Numbers	*A* 9	*B* 6	*C* 2	*D* 10	*E* 8	*F* 1	*G* 5	*H* 4	*I* 7	*J* 3	Errors Unclassified	 x	 *	 Total
First series	3 5 1 2 9	8 7 10 3 1 6	5 3 1 7 10 9x 4 2	5 7 10	8	7 5 4 1	10x 3 5	10x 3 1x 7 6x 9x 2x 7* 9x* 2x* 4	5x 3 7	3	35	10	3	48
Second series	2 6 8 4 1 9	6	10 9x 2	9x 7 10	7 9x 8	7 5 3 9x 1	7 3 5	5x 9x 7 5x* 7* 9x* 10x 6x 9x* 6x* 4	7	9x 10x 6x 8x 2x 1x 3	33	18	5	56
Third series	8 6 9	2 4 8 10 1 7 8* 6	10 1 8 6x 3 7 5 8* 6x* 5* 4 10* 2	10	7 5 3 1 8	1	7 3 4 2x 5	4	3 7	3	30	3	5	38
Fourth series	9	6	2	10	8	1	5	4	7	3	0	0	0	0
Fifth series	9	6	1 8 2	10	8	1	5	4	7	3	2	0	0	2
Sixth and seventh series correct														
U.C. Errors	11	12	23	4	6	7	8	20	3	6	100			
x Errors	0	0	4	1	1	1	2	15	1	6		31		
* Errors	0	1	4	0	0	0	0	8	0	0			13	
Totals	11	13	31	5	7	8	10	43	4	12				144

particular letters associated with them." He evidently tried to disregard, or to exclude from attention, all his wrong guesses and to hold only the right ones. This principle was adopted in the third series, and the results show themselves in the fourth. In this case, then, we should get a sudden drop in the error curve of learning due to the adoption of this efficient idea. From this point on pure mechanical memory must do the rest, within the range set for it.

A somewhat similar but less mechanical procedure was

Table III

Record of Miss A. R., a Graduate Student

Letters / Numbers	*A* 9	*B* 6	*C* 2	*D* 10	*E* 8	*F* 1	*G* 5	*H* 4	*I* 7	*J* 3	Errors: Unclassified	Errors: x	Errors: *	Errors: Total
First series	10 9	10 9x 8 7 6	10 9x 8 7 6x 5 4 3 2	1 2x 3 4 5 6x 7 8 9x 10	1 2x 3 4 5 6x 7 8	1	1x 2x 3 4 5	1x 2x 3 4	1x 2x 3 4x 5x 6x 7	1x 2x 3	44	19	0	63
Second series	9	6	2	8 10	8	1	5	4	7	3	1	0	0	1
Third series	9	6	2	8 10	8	1	5	4	10x 7	3	2	1	0	3
Fourth series	10 9	6	2	8 10	8	1	5	4	7	3	2	0	0	2
Fifth series	9	6	2	8 10	8	1	5	4	7	2x 3	2	1	0	3
Sixth series	9	6	2	10	8	1	5	4	7	3	0	0	0	0
Seventh series	9	6	2	10	8	7 1	5	4	7	3	1	0	0	1
Eighth and ninth series correct														
U.C. Errors	2	4	8	13	7	1	4	3	7	3	52			
x Errors	0	1	2	3	2	0	2	2	6	3		21		
* Errors	0	0	0	0	0	0	0	0	0	0			0	
Totals	2	5	10	16	9	1	6	5	13	6				73

TABLE IV

MR. L. E., A UNIVERSITY PROFESSOR

											Errors			
Letters Numbers	*A* 9	*B* 6	*C* 2	*D* 10	*E* 8	*F* 1	*G* 5	*H* 4	*I* 7	*J* 3	Unclassified	x	*	Total
First series	1 2 3 4 5 6 7 8 9	1 2 3 4 5 6	5 4 3 2	1 3 4 8 10	1 3 4 5 7 6x 9x 10x 8	1	3 4 5	4	3 7	3	31	3	0	34
Second series	9	7 6	2	10	3 1 4 7 5 8	3 1	5	4	3 7	3	8	0	0	8
Third series	9	6	2	8 10	7 5 4 8	1	7 5	4	7	3	5	0	0	5
Fourth series	1 9	6	2	10	1 8	1	5	4	7	3	2	0	0	2
Fifth series	9	6	10 2	10	8	1	5	4	7	3	1	0	0	1
Sixth and seventh series correct														
U.C. Errors	9	6	4	5	17	1	3	0	2	0	47			
x Errors	0	0	0	0	3	0	0	0	0	0		3		
* Errors	0	0	0	0	0	0	0	0	0	0			0	
Totals	9	6	4	5	20	1	3	0	2	0				50

adopted by the subject Miss A. R., a graduate student. She succeeded in the first series in finding a rational order in the numbers, but likewise wholly disregarded at the same time the possible minimizing of errors by avoiding the guessing of numbers already used. So while the limitation of attention in one sense aided the learning, in another it failed to prevent a certain kind of important errors. The procedure was therefore not highly efficient. At the end of the experiment Miss A. R. reported that she noted this order: "9, 6,

2,—beginning with 9 the numbers decrease by three, except that 2 is one step too low; then come 10 and 8, two less, and 1, the smallest number; 5, the next, is always easy to remember, and 4, one less, follows. I learned 7 and 3 directly." This scheme, however, did not prevent continued errors on numbers thus rationally arranged. Note, for instance, the four-times-repeated error of guessing 8 for 10 with *D*. Evidently the rationalization must not have occurred all at the first series, but gradually. The result was a large number of series with comparatively few errors. It should be noted, however, that the methods used by L. D. and A. R. give a high proportion of logical errors.

A. R. made no perseverative errors. This is due to the fact that while looking for the relations of the successive numbers, already pointed out, she followed rationally in the first random guesses, the definite order either of beginning with 1 and numbering up in order, or, taking the reverse course, of beginning with 10. She therefore followed a more thoroughly rational order than did L. D. and made fewer errors; but she allowed attention to be distracted from another useful point—that of leaving out, in this definite order of procedure with each letter, numbers already used with preceding letters. She continued to repeat, after using them, 9, 6, 2, and so on.

This particular precaution was taken by the final illustrative case we shall consider, along with the other precaution of guessing from 1 upward in the number series with each letter. Mr. L. E., the subject, is a university professor of good native ability and very careful logical training. The entire situation was, however, not grasped in the first series, and as a consequence three logical errors occur in the guessing of the number of *E*. After this point the mastery of the problem was wholly one of memory, the logical situation having been grasped and kept in mind; and the field for memory to master was greatly limited so that the problem was soon solved. Little slips of memory, however, made necessary seven repetition series for the solution of the problem as defined. In this case we have a small proportion of logical errors to

total errors, and also a relatively small proportion of logical errors to the total number of repetition series required. Only one student excelled L. E.'s record, and this happened to be the only high school student in our present group, a boy of an abnormally high intelligence quotient (IQ = 1.45). This subject, R. B., however, made twice as many logical errors as L. E., therefore excelling him only in retention.

It is very difficult to lay down any definite rule as to the best logical procedure in a problem of this kind, for many of the best subjects follow original plans of their own. Thus the subject with fewest errors of all, R. B., neglected the rule to guess in order from 1 to 10 but just struck out in any order. The problem seemed to be so simple for him that he kept the situation well enough in mind so as not to make any perseverative errors. It is possible that by following the numerical order in the guessing, he would have been freer to avoid logical errors, as was the case with L. E. On a harder problem, *i. e.*, one with, say, twenty or more letters, the advantage of such a procedure would probably have shown itself perceptibly.

We shall now present tables of the results of all the subjects in the first group of our experiment, subjects who were not definitely told that time would be a criterion in judging of their efficiency. These tables include also the results already considered individually. Time considerations were omitted in order that the subjects might not feel under too much pressure for the amount of deliberation that they would naturally be inclined to make. A separate report will give the results of subjects encouraged to complete the learning in as little time as possible.

Table V gives the summarized results of nineteen subjects classified as follows: Two university instructors, two graduate students, ten college students, one high school student, and four other adults,—one electrical engineer, one doctor, one woman of 50 years who had completed part of a high school course, and one bond clerk, a high school graduate. The instructors are L. E. and L. H.; the graduate students are A. R. and P. N.; R. B. is the high school student; and the

Table V

Errors in Successive Series

Subject	1st			2d			3d			4th			5th			6th			7th			8th			9th		
	Uc	L	P	Uc	L	P	Uc	L	P	Uc	L	P	Uc	L	P	Uc	L	P	Uc	L	P	Uc	L	P	Uc	L	P
A. B.	36	9	3	32	14	2	43	13	5	14	2	0	3	3	0	6	6	0	0	0	0	9	8	1	0	0	0
L. D.	35	10	3	33	18	5	30	3	5	0	0	0	2	0	0	...	...	...	...	...	...	...	...	...	...	...	...
A. R.*	44	19	0	1	0	0	2	1	0	2	0	0	2	1	0	0	0	0	1	0	0	...	...	...	...	...	...
L. E.	31	3	0	8	0	0	5	0	0	2	0	0	1	0	0	...	...	...	...	...	...	...	...	...	...	...	...
T. R.*	33	10	1	22	15	5	7	1	0	20	2	2	4	2	0	1	0	0	0	0	0	1	0	0	1	0	0
G. R.*	30	0	0	8	3	0	11	0	0	5	1	0	3	0	0	...	...	...	...	...	...	...	...	...	...	...	...
F. H.*	40	14	0	30	15	4	46	18	9	32	14	0	23	7	1	30	10	10	13	5	0	3	1	0	3	2	0
M. B.	37	5	0	33	17	1	21	8	0	21	8	0	30	9	2	10	4	0	1	0	0	...	...	...	...	...	...
F. W.	24	5	1	16	7	1	22	2	7	1	0	0	2	0	0	...	...	...	...	...	...	...	...	...	...	...	...
A. E.	29	2	1	15	5	1	9	2	2	1	0	0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
M. E.*	34	3	0	45	21	20	16	11	2	3	1	0	12	5	3	11	3	3	3	2	0	0	0	0	2	1	0
P. E.	22	4	0	4	2	0	4	0	0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
H. R.	28	7	0	28	5	0	30	12	4	31	11	3	14	8	0	15	8	1	7	2	0	8	1	0	5	1	0
L. H.	29	4	0	8	5	0	13	4	0	17	3	0	13	2	0	...	...	...	...	...	...	...	...	...	...	...	...
R. S.*	29	7	0	28	10	1	42	21	5	20	7	0	14	5	0	7	2	0	6	2	0	11	3	0	1	0	0
O. E.*	48	27	3	52	17	12	31	14	2	47	28	8	19	9	0	16	6	0	26	10	1	29	16	3	20	4	2
M. C.*	45	20	5	77	44	21	52	17	12	46	23	8	17	6	2	23	7	7	6	0	0	0	0	0	1	0	0
C. N.*	50	19	0	42	25	1	30	14	0	30	16	0	22	13	0	21	12	0	15	6	0	16	8	0	12	9	0
P. N.	29	8	0	48	19	15	14	5	0	12	3	1	3	0	0	1	0	0	...	...	...	...	...	...	...	...	...
Totals	653	176	17	540	242	89	428	146	53	303	119	22	184	70	8	141	58	21	78	27	1	77	37	4	45	17	2
	846			871			627			444			262			220			106			118			64		

TABLE V—*Continued*

Subject	10th			11th			12th			13th			14th			15th			16th			17th			Totals		
	Uc	L	P	Uc	L	P	Uc	L	P	Uc	L	P	Uc	L	P	Uc	L	P	Uc	L	P	Uc	L	P	Uc	L	P
A. B.	5	2	0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	148	57	11
L. D.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	100	31	13
A. R.*	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	52	21	0
L. E.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	47	3	0
T. R.*	2	0	0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	101	30	8
G. R.*	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	57	4	0
F. H.*	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	220	86	24
M. B.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	153	51	3
F. W.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	65	14	9
A. E.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	54	9	4
M. E.*	1	1	0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	127	48	28
P. E.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	30	6	0
H. R.	1	0	0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	166	55	8
L. G.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	80	18	0
R. S.*	5	2	0	1	0	0	0	0	0	1	0	0	5	0	1	...	...	...	...	...	...	...	...	...	170	59	7
O. E.*	25	10	2	9	4	0	6	0	0	5	1	0	0	0	0	1	0	0	1	0	0	...	...	...	335	146	33
M. C.*	4	0	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	271	117	56
C. N.*	10	3	0	9	4	0	7	3	0	24	10	5	4	2	0	3	1	0	2	0	0	1	1	0	298	146	6
P. N.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	107	35	16
Totals.	53	18	3	19	8	0	13	3	0	30	11	5	9	2	1	4	1	0	3	0	0	1	1	0	2581	936	226
	74			27			16			46			12			5			3			2			3743		

four persons last described in the preceding sentence are M. B., A. E., F. H., and F. W., respectively. In the table females are marked with an asterisk (*).

Figure I shows in graphic form the average number of errors made in successive repetition series by the nineteen

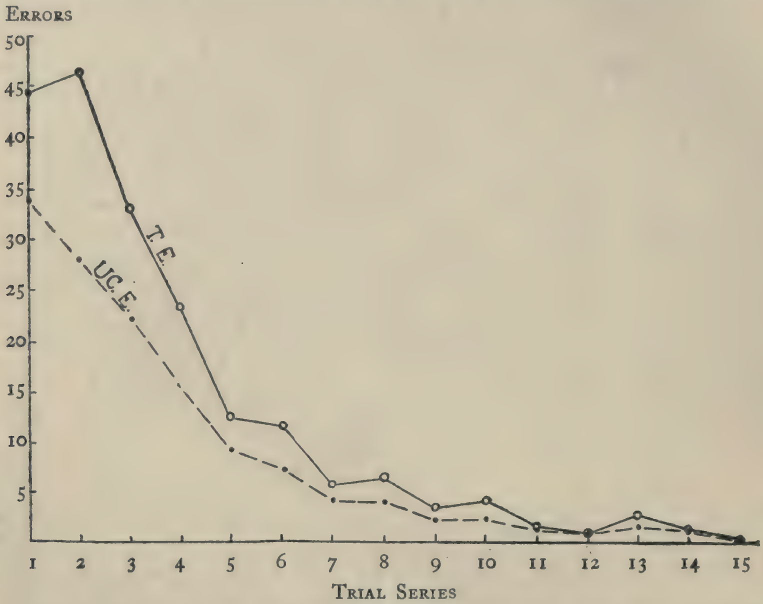

Fig. I. Rational Learning Curve, based on Averages of 19 subjects. UC.E. is the Curve of Unclassified Errors; T.E., that of the Total Errors. Each Trial Series Consists in Guessing Numbers for A, B, C, . . . J.

subjects. UC.E. is the curve of the unclassified errors and T.E. is that of the total errors determined as has been explained. Which of these curves is the more accurately representative of the learning is hard to say without more extended comparisons and analyses. The curve T.E. results from giving greater weight to certain errors than to others. This is true in a measure even of the curve UC.E., for the fewer the logical and the perseverative errors a subject makes the more he is able to limit his field of guess-trials and thus to reduce his errors. These curves in themselves are not very illuminating as they conceal the points of greatest significance in

the learning. The same thing is true to a large extent of the individual curves, which the interested reader may plot for himself from the results as given in Table V.

In this case the error curves should not be regarded as comparable to most error curves of learning without certain qualifications. It has been shown in another article that if the thing to be accomplished successively in different trials be regarded as a constant, K, we have an equation of the form $K = ae$, or $K/a = e$, wherein e represents the errors and a the average attainment of some sort, depending on the nature of the experiment; and that when K is large the e-curve drops relatively very rapidly at first, for purely mathematical reasons, while it shows a more gradual decline when K is small.[1] In our present case K is very small ($= 10$) and we should therefore expect a slow decline of the error curve.

Among the features in which this learning differs from the maze learning, for instance, is this one in particular: when the individual turns back in the maze before reaching the food box or the exit he lays himself liable to certain additional possibilities of error (of entrance to blind alleys already passed) which are avoided as soon as he learns to keep the forward direction. Thus when the subject learns to keep the forward direction, which even the rat in the maze does considerably before it learns to avoid entering blind alleys,[2] he suddenly decreases his number of errors by eliminating at once all those that would have been made had he returned in the maze and been again obliged to pass the blind alleys already passed to the point in question. This is one of the causes of the sudden drop in the error (and time, and excess distance) curve in maze learning, which cause is absent in the present case of rational learning.

It is, however, significant that in this case of rational learning only so few subjects master rather suddenly and noticeably the rational aspects of the situation; thus only a few show a rather significant drop in the error curve or in the total number of errors. The better subjects, and some of

[1] Jos. Peterson, 'Experiments in Ball-Tossing: The Significance of Learning Curves,' J. Exp. Psychol., 1917, 2, 178–224.

[2] See *Behav. Monog.* No. 15, 1917, 24 f., by the writer.

those finding greater difficulty with the problem, show such a drop; but, simple as the problem is, one is not at all safe in concluding from the absence of such a drop that ideas and a degree of rationalization were not employed. It is the particular choices and the numbers avoided by the subject, as shown in his detailed record, that indicate most clearly the degree of reason employed and its exact nature. This fact indicates very strongly that any hasty or general conclusion arrived at from the curves or from general averages, as is too frequently done in studies of learning, is in a high degree unreliable; to understand learning better, as the writer has urged in some of the previous articles already cited, we must take the pains to study its detailed phases. A subject may try one idea and drop it for another; and our present experiment indicates that when the individual reactions are considered, we find many different but rather effective methods used. Yerkes found "that *where very different methods of learning appear, the number of trials is not a safe criterion of intelligence.*"[1] In general, of course, the number of trials and the number of errors do indicate in the present study the degree of rationalization of the reactions; but the kind of error is far more significant.

The writer feels, much more than this rather general report indicates, that the present type of experiment suggests to the person applying it to a subject a number of very significant aspects of the subject's type of mind, traits that are of considerable importance in the selection of individuals for different kinds of work. Individuals of about the same amount of mental ability, as measured by intelligence tests, seem to differ very significantly in some of these traits. And it seems to the writer that by the addition of better controls, such as the use of a means of recording the exact time-relations of the several responses, a very good objective record can be obtained of most of these significant traits. Even by the present method—and it has been the purpose to reduce the technique to as simple a matter as possible—most of these traits show in the individual records.

[1] *Behav. Monog.* No. 12, 1917, p. 68.

Some of the subjects show a cold deliberateness that is not easily disturbed. For them the problem is an intellectual situation to be solved, and they keep cool and considerate of the various factors involved in attempting the solution. There is less evidence of great interest in one's own *personal* record, of the desire to "show up well"; the interest is in finding what people in general will do with the problem. This is the more objective, rational type. Miss G. R. is one of the best examples of this type.

Then there are the individuals of a more subjective turn of mind, who easily get confused and allow their attention to be narrowed down, by emotional disturbances and fear of making a bad record, to irrelevant self-thoughts. The result is that the real situation before the subject is neglected in some of its essential aspects, and we get the inconsistent guessing of numbers which the subject knows perfectly well have been used for other letters but he "didn't think about it." Persons of this kind are rather nervous and uneasy in the test and seem over-concerned about any particular error and keep it in mind to the detriment of subsequent guesses. Any tendency to dwell too much on the past at the expense of the present demands on one's attention is something of a dissociation, and borders on the abnormal. It may, of course, manifest itself in any normal person under peculiar conditions, but some are much more prone to it than are others. It is reasonable to suppose, and abnormal psychology gives many evidences of the correctness of the supposition, that such persons cannot stand the strain that those of the first type can. M. E., H. R., R. S., O. E., and C. N. are of this type. Such persons are considerably concerned about the attitude of others toward them and they probably need more personal motives and attention to do their best work, if this is not carried to the point of confusion.

A third type of individual, best represented by a member of another group of persons tested, strikes out boldly by an almost pure trial and error procedure. The person referred to, a man devoting part of his time to business in the city and part to university work, proceeded at once to guess rapidly

for each letter, seemingly little concerned about errors. It took him an abnormally long time to complete the learning and he made a large number of errors, with a high proportion of logical but a small proportion of perseverative errors; but this did not greatly disturb him as it would have done those of the second type already considered. When he got through I asked: "Are you not a sort of hit-or-miss type of individual in your work and methods of thinking?" He thought a little, and replied that that is a rather good characterization of him. A time record of one's responses would show up this trait better than the records we have show it. The average rate of response can, of course, be derived by dividing the total time by the total number of responses.

Another general classification of certain extreme individuals would put them into classes of aggressive, vigorous attack on the problem, on the one hand, and of so much passivity on the other hand that principles can hardly be discovered. This is a different sort of classification from the one above; the active person, *e. g.*, might be either logical and efficient at the test or self-conscious and confused. One individual, M. C., went through the problem very passively and unconcerned, and had considerable difficulty in mastering it. When finally she succeeded I asked her if she saw any room or opportunity for reasoning the matter out and of thus aiding memory. The reply was: "I don't know that I do." This same sort of passivity characterizes her work in psychology, though she can do nearly average work with the necessary effort.

So much for our venture in 'character reading.' It, of course, remains to be demonstrated that different individuals would characterize these persons, respectively, in the same way, with a fair degree of reliability. Possibly, also, other lines of cleavage are more natural than those suggested. Hamilton found, somewhat in conformity with the present results, that 'excitability' and 'distractability' tended to dispose animals to his reaction types D and E. Type D is defined as "Response by entering a given alley more than once during a given trial, with an interval of effort to escape by

another alley between entrance and reëntrance of the same alley." In type E the animal showed either two or more efforts to escape by some alley or group of alleys during a given trial without intervals of effort to escape by other alleys, or a persistent avoidance of one alley while trying others six or more times.[1]

Rankings on the basis of fewness of errors in this test show with the few subjects here employed a correlation of .86 with the estimated intelligence of the subjects, and of .96 with rankings based on six technical examinations in the general psychology course. The latter correlation is, however, based on only seven cases. Correlations based on a large number of cases will be given in a subsequent article. The present indication is that the rational learning experiment is a rather good test for intelligence, though it obviously has other important uses.

Summary

1. A simple experiment without complex apparatus is devised which enables the experimenter to record objectively with a high degree of accuracy all the relevant reactions of the subject solving the problem in rational learning.

2. The difficulty of the problem can be increased or decreased to any desirable degree, without interfering with the general principle of the experiment. The problem can therefore easily be adapted to human subjects of various intellectual capacities.

3. In a problem in rational learning of this kind it seems to be unsafe to lay down any particular method as the best method of procedure. Some of the best subjects depart from our 'best method' and solve the problem in least time and with fewest errors. With greater complexity in the problem, however, easily provided for in the present learning test, it is probable that the orderly procedure suggested as the best method would show superiority over other procedures.

4. Curves of error in rational learning, when the situation is not too simple, do not show important differences from

[1] G. V. Hamilton, 'A Study of Perseverance Reactions in Primates and Rodents,' *Behav. Monog.* No. 13, 1917, 44 f.

other error curves of learning. The absence of sudden drops in the curve does not prove that the subject did not have, or did not use, ideas; a better criterion of the use of ideas is the consistency of the individual choices of any particular subject.

5. The present rational learning test seems to promise something in the way of an analysis of different individual traits, more or less independent of what is ordinarily regarded as intellectual ability,—such as (1) cold rationalistic tendencies, (2) extreme self-consciousness and a disposition to confusing, emotional excitement, (3) a tendency to a rough trial-and-error procedure with little concern as to errors made, a sort of hit-or-miss type of mind motivated by an abundance of energy poorly directed; and also other classifications of subjects, not coördinate with these, into aggressive alertness in attack on problems, on the one hand, and on the other an extreme passivity, one that does not permit of selection to any adequate extent and that tends too much toward mere contiguity associations not fruitful in the learning.

6. With the few subjects used a very high correlation is obtained between performance in the rational learning and estimated general intelligence or standing in general psychology based on technical examinations.

7. On occasions of mental confusion there is a surprising neglect of certain pertinent elements in the situation reacted to, sometimes called a narrowing of consciousness. Often with very little confusion noted, subjects report that they 'didn't think' of facts which they had learned and had recently used.

8. Rational learning does not seem to differ from the usual trial and error learning in any important manner except in the explicitness with which the various elements in the situation are reacted to and retained for subsequent use. It is probable that in all forms of learning, even in those forms showing no errors, as errors are usually conceived, there is a struggle between various impulsive tendencies and that, by means of overlapping neuro-muscular excitations, inhibitions and facilitations occur which serve as selective agencies and

determine, with other circumstances, the connections formed in the learning. Thus an anticipatory adjustment is possible even though there are no anticipatory ideas or conscious purposes, and the acts most consistent with the total conditions, extra-organic and intra-organic, survive in the learning over those which are more or less conflicting. Empirical justifications of the statements in this paragraph will be given in a later report.

A CONTRIBUTION TO THE EXPERIMENTAL STUDY OF ANALOGY

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Introduction

The researches of Thumb and Marbe[1] on the psychological basis of analogy[2] in language were an attempt to determine what characteristics a word-association must possess in order to become linguistically effective. Their investigation established the following characteristics:

1. The effectiveness of an association in producing analogical changes increases with its *frequency*, *i. e.*, with the number of members of a speech-community in whom it is present. This may be represented algebraically by $E = F/n$ when E represents the effectiveness in producing analogical changes, F the frequency or number of occurrences of the association and n the number of persons under investigation.

2. The effectiveness of an association increases with a decrease in the average *time* required for the association to take place. This may be represented algebraically by $E = 1/(T + 1)$ when T represents the average association-time; with a decrease in the association-time the value of E approaches 1.[3]

Strictly speaking the results of Thumb and Marbe's in-

[1] A. Thumb und K. Marbe, 'Experimentelle Untersuchungen über die psychologischen Grundlagen der sprachlichen Analogiebildung,' Leipzig, Engelmann, 1901.

[2] By 'analogy' and 'analogical changes' we mean those changes in the form or meaning of words or groups of words which are brought about through association with other words or groups of words.

[3] In a further investigation, published in *Indogermanische Forschungen*, 22, 1–55, Thumb established an additional characteristic for linguistically effective associations, namely their 'spontaneous' nature. In this type of association the associated word follows immediately after the stimulus-word without any intervening mental process. In my experiments the association-types were not investigated, for reasons set forth below.

vestigation are applicable only to the German Language; or still more strictly only to the dialect of his observers.

The Problem.—Briefly stated my problem was (1) to make a beginning in the establishing of the associational basis of the English language and (2) to make a comparative study of the English and German results.

Experimental Investigation

In the first experiments of Thumb and Marbe, a list of 60 words was chosen from different categories as follows: 10 names expressive of family relations, *e. g.*, father, mother, etc., 10 adjectives, 10 numerals from 1 to 10, 10 pronouns, 10 adverbs of place, 10 adverbs of time. All sixty of these words were presented to each observer at one sitting. They were arranged in such an order than in no case would a word immediately follow another of the same category; miscellaneous words not belonging to the list selected for the experiment were frequently interposed. The observers were eight in number, all students, teachers, or doctors of philosophy. In a later experiment 80 verbs (infinitives) were used as stimulus words. Eight observers were again used, three of whom (students) had not served in the first experiment. A stop-watch was used to measure the reaction-time in the first experiment and a Hipp chronoscope and voice key in the later investigation. The results of the two investigations do not differ materially from each other. In very few cases do the favored associations of the one investigation differ from those of the other.

In my experiments, conducted in the psychological laboratory of the Ohio State University in the spring and summer of 1917, 126 observers were used. These fall into three groups. The members of Group A (100 students) are classified in the following table according to their standing as represented in the table. The four vertical columns, numbered 1, 2, 3, 4, refer to the four undergraduate classes; the remaining columns, designated by Roman numerals, refer to the following classes: I, graduate students; II, professors; III, a group of high school teachers who attended the Uni-

versity Summer School of 1917; IV, a group of elementary school teachers who attended the same summer school.

	1	2	3	4	I	II	III	IV	Total
Men..........	14	3	5	3	5	2	21	2	55
Women.......	6	10	6	3	3		9	8	45

Group B consisted of 11 children, aged 9 to 13 years. Group C consisted of 15 university janitors. By means of Group A, I desired to test out the results of Thumb and Marbe with a large group of observers, as compared with the very small group used by the German investigators. This large group was supplemented by the other two groups in order that the results gained from educated adults might be compared with those from uneducated adults and children.

The stimulus-words used were made to correspond as nearly as possible to those used by Thumb and Marbe in their earlier experiments. They fall into six categories, as follows: 10 names of personal relations, 10 adjectives, 10 numerals (1 to 10), 10 verbs (present participles), 10 pronouns, 10 adverbs of time and place. The participles of verbs were used to prevent verbs such as *walk*, used in the infinitive, from being understood as nouns. Only one- and two-syllable words were used, to avoid the possibility of the reaction time being increased by the length of the stimulus-word. Hence no translation of German *Schwager*, etc., was used. A preliminary experiment had been undertaken to discover whether there were any marked difference in length between the reaction-times when one-syllable stimulus words were used and when two-syllable words were used. The results showed the time for the two-syllable words practically the same as for the one-syllable words; the difference may therefore be regarded as negligible for our purposes.

The method employed was as follows: a Hipp chronoscope was set up in a room adjoining that in which the observer and experimenter sat. This was to prevent the sound of the chronoscope from disturbing the observer. The experimenter controlled the chronoscope by means of a key

which was connected by wiring with the instrument. The chronoscope readings were taken by an assistant.[1]

The following instructions were read to each observer:

Please keep your eyes closed during the experiment. I will read you a list of words. To each word respond as quickly as possible with another word. The first five words will be for practice.

I then gave the following words: *house*, *jumping*, *fast*, *red*, *who*. The words chosen for the experiment then followed. These were arranged in ten different haphazard orders, so that in each ten observers no one was given the stimulus-words in the same order. As the experimenter began the articulation of each word, he pressed the key which started the chronoscope; when the observer began to respond the experimenter released the key, thus stopping the chronoscope. (The experimenter did not look at the observer during the experiment, but released the key upon receiving the auditory stimulus of the observer's response.)

No attempt was made to classify the associations according to the occurrence or non-occurrence of mental processes intervening between the stimulus and reaction words, because this would have required introspection on the part of the observers, whereas it was my desire to make the conditions of the experiment as simple as possible, thus approaching more nearly the conditions in everyday speech. Moreover, as Thumb himself notes,[2] the 'spontaneous' character of an association may be inferred from the rapidity with which it occurs; the associations having the shortest reaction-times are the 'spontaneous' ones.

In the following tables, the results for Group A are presented. The figures in parentheses, placed after the stimulus-words, indicate the total number of successful reactions. The columns under *n*, *A* and *M* give the number of occurrences, the average reaction-time and the median[3] of the re-

[1] Miss Ethel M. Cooke, a student in the university, to whom I am greatly indebted for her careful and patient work.

[2] *Indogerman. Forsch.*, **22**, 18, 24.

[3] The median is to be regarded as more significant than the average because it is not affected by the extremely long or short reaction-times. We are concerned, not so much with the *average* times as with the *most frequent* times.

TABLE I

Stimulus	Most Frequent Reaction				Next Most Frequent Reaction				All Remaining Reactions		
		n	A	M		n	A	M	n	A	M
1. *father........(95)	*mother.......	69	1.265	1.160	son...........	4	1.266	1.248	22	2.229	1.904
2. *mother.......(96)	*father........	55	1.310	1.219	sister..........	7	1.300	1.289	34	2.321	1.976
3. *son[1].........(44)	daughter......	27	1.443	1.223	father.........	7	2.129	2.124	10	2.155	2.044
4. *daughter.....(94)	son..........	35	1.603	1.347	sister	15	1.404	1.212	44	2.181	1.765
5. *brother......(94)	*sister.........	72	1.165	1.060	relative[2]	3	2.087	1.437	19	2.479	2.313
6. *sister........(98)	*brother.......	72	1.208	1.146	girl...........	4	2.364	2.214	22	1.989	1.908
7. cousin.......(97)	uncle.........	20	1.623	1.314	aunt..........	19	2.308	1.843	58	2.038	1.960
8. aunt ....(89)	uncle.........	67	1.327	1.290	Mary.........	4	2.038	1.700	18	2.060	1.894
9. uncle........(99)	aunt.........	66	1.335	1.228	relative[3].......	4	1.648	1.596	29	2.021	1.870
10. nephew......(96)	niece.........	58	1.600	1.472	cousin.........	10	1.680	1.698	27	2.330	1.931
1. Vater.........(8)	Mutter.......	5	1.24		——	–	—		3	2.13	
2. Mutter...........	Vater........	3	1.67		meine M.......	2	3.00		3	1.93	
3. Sohn.............	Vater.........	5	1.36		Tochter.......	2	2.10		1	3.80	
4. Tochter..........	Mutter.......	4	1.50		——	–	—		4	1.70	
5. Bruder...........	Schwester.....	6	1.33		——	–	—		2	4.30	
6. Schwester........	Bruder.......	4	1.90		——	–	—		4	1.60	
7. Vetter...........	Base.........	3	1.40		Schwester......	2	1.60		3	2.67	
8. Base.............	Vetter........	5	1.88		——	–	—		3	3.27	
9. Schwager.........	Bruder.......	2	1.60		——	–	—		6	2.70	
10. Schwägerin.......	——	–	—		——	–	—		8	2.25	

[1] The small number of successful reactions to the stimulus 'son' is due to the fact that many observers understood it as 'sun.'

[2] Including 1 case of 'relation.'

[3] Including 2 cases of 'relation.'

Table II

Stimulus	Most Frequent Reaction				Next Most Frequent Reaction				All Remaining Reactions		
		n	A	M		n	A	M	n	A	M
1. *big (93)	*little	52	1.150	1.079	small	18	1.208	1.132	23	1.922	1.599
2. *little (93)	*big	52	1.280	1.224	small	17	1.458	1.309	24	2.073	1.811
3. *light[1]											
4. *heavy (96)	*light	73	1.356	1.272	load	6	1.450	1.455	17	2.479	1.839
5. *old (93)	*young	69	1.382	1.223	new	7	1.105	1.124	17	2.200	2.201
6. *young (97)	*old	80	1.179	1.102	man	4	1.348	1.301	13	2.872	2.186
7. *thick (99)	*thin	72	1.346	1.214	heavy	4	1.538	1.589	23	2.620	2.030
8. *thin (93)	*thick	50	1.351	1.217	fat	17	1.499	1.427	26	2.089	1.688
9. *white (96)	*black	74	1.181	1.128	dark / light	3 / 3	1.216 / 1.632	1.078 / 1.772	16	1.969	1.694
10. *black (98)	*white	79	1.173	1.097	color	3	2.314	1.693	16	2.059	2.054
1. gross (8)	klein	7	1.29						1	2.40	
2. klein	gross	6	1.37						2	2.00	
3. leicht	schwer	7	1.46						1	2.00	
4. schwer	leicht	6	1.23						2	1.90	
5. alt	jung	6	1.30						2	2.40	
6. jung	alt	7	1.17						1	2.20	
7. dick	dünn	7	1.26						1	2.00	
8. dünn	dick	7	1.29						1	2.00	
9. weiss	schwarz	7	1.63						1	1.60	
10. schwarz	weiss	6	1.43						2	2.50	

[1] Out of a total of 97 observers who responded to the stimulus 'light,' only 4 understood it in the sense intended, *i.e.*, German 'leicht,' 'not heavy.' The results are as follows:

	Most Frequent Reaction	Av. Time	Av. Time of Other Reactions
1. Understood as 'not heavy' (4)	heavy (4)	1.299	——
2. Understood as 'not dark' (58)	dark (54)	1.281	1.802
3. Understood as a noun (35)	darkness (6)	1.251	2.038

Table III

Stimulus	Most Frequent Reaction				Next Most Frequent Reaction				All Remaining Reactions		
		n	A	M		n	A	M	n	A	M
1. *one..........(97)	*two..........	78	1.160	1.102	——	–	—	—	19	2.316	2.030
2. *two..........(92)	*three.........	56	1.250	1.127	four	13	1.440	1.377	23	1.783	1.546
3. *three........(98)	*four..........	68	1.163	1.084	number	9	1.599	1.515	21	1.749	1.515
					three	5	1.486	1.385			
4. *four[1].........(83)	*five..........	53	1.168	1.096	six	5	1.304	1.308	15	2.116	2.021
					number	5	2.893	2.214			
5. *five[1].........(87)	*six...........	55	1.296	1.254	ten	8	1.577	1.398	24	1.817	1.653
6. *six..........(94)	*seven.........	65	1.102	1.044	number	5	3.104	2.154	24	1.954	1.828
7. *seven........(99)	*eight.........	68	1.220	1.114	number	6	1.580	1.410	25	2.122	1.747
8. *eight........(91)	*nine..........	66	1.183	1.090	ten	5	1.023	1.179	20	1.814	1.626
9. *nine.........(97)	*ten...........	73	1.352	1.186	number	5	1.831	1.544	19	2.198	2.157
10. *ten..........(97)	eleven........	51	1.219	1.131	twenty	8	1.599	1.562	38	1.946	1.549
1. eins..........(8)	zwei..........	5	1.20		——	–	—		3	2.13	
2. zwei.............	drei..........	4	1.15		——	–	—		4	1.75	
3. drei.............	vier..........	5	1.32		——	–	—		3	2.00	
4. vier.............	fünf..........	6	1.13		——	–	—		2	2.20	
5. fünf.............	sechs.........	6	1.17		——	–	—		2	6.70	
6. sechs............	sieben........	5	1.16		——	–	—		3	2.00	
7. sieben...........	acht..........	6	1.33		——	–	—		2	2.20	
8. acht.............	neun..........	6	1.43		——	–	—		2	6.00	
9. neun.............	zehn..........	5	1.52		——	–	—		3	1.87	
10. zehn.............	zwanzig......	3	1.60		elf	2	1.20		3	2.33	

[1] The relatively small number of successful reactions to the stimulus words 'four' and 'five' is due to many observers understanding these as 'for' and 'fife' respectively.

TABLE IV

Stimulus	Most Frequent Reaction				Next Most Frequent Reaction				All Remaining Reactions		
		n	A	M		n	A	M	n	A	M
1. *giving.......(98)	*taking........	39	1.396	1.306	receiving.....	7	1.473	1.489	45	2.052	1.727
					money.......	7	1.925	1.861			
2. *taking.......(93)	*giving........	16	1.626	1.400	receiving.....	8	2.090	1.895	69	2.150	2.009
3. *eating.......(94)	*drinking......	42	1.446	1.332	food.........	11	1.933	1.724	41	2.056	1.642
4. *drinking.....(95)	eating........	30	1.863	1.815	water.......	25	1.632	1.453	40	2.069	1.593
5. *losing[1].......(81)	finding.......	29	1.459	1.422	gaining......	9	1.368	1.288	34	1.990	1.499
					lost.........	9	1.627	1.478			
6. *walking......(97)	*running.......	29	1.547	1.376	riding.......	17	1.486	1.464	51	1.826	1.665
7. *reading......(96)	*writing.......	55	1.298	1.200	book.........	18	2.108	1.810	23	2.358	1.743
8. *writing.......(97)	*reading.......	43	1.729	1.674	letter(s).....	8	1.722	1.692	46	1.831	1.722
9. seeking[1]......(83)	finding.......	28	1.490	1.399	looking[2].....	8	1.712	1.634	47	2.013	1.838
10. *finding.......(92)	losing........	20	1.915	1.699	lost.........	10	1.566	1.574	62	2.174	1.883
1. geben.........(8)	nehmen.......	4	1.75		——	–	—		4	2.30	
2. nehmen..........	geben........	6	1.33		——	–	—		2	1.80	
3. essen.............	trinken.......	6	1.13		——	–	—		2	1.90	
4. trinken...........	winken.......	2	1.40		——	–	—		6	1.37	
5. verlieren.........	Geld.........	6	1.90		——	–	—		2	1.90	
6. gehen............	laufen........	2	1.30		——	–	—		6	1.93	
7. lesen.............	schreiben.....	5	1.16		Buch........	2	1.70		1	2.00	
8. schreiben.........	lesen.........	4	1.15		——	–	—		4	3.50	
9. ——[3]											
10. finden...........	suchen.......	4	1.40		——	–	—		4	1.50	

[1] The stimulus words 'losing' and 'seeking' were not used for the first 14 observers; hence the smaller number of responses.

[2] 'hunting' occurs 7 times (A, 1.486 M, 1.416); 'hunt' occurs 3 times (A, 2.192 M, 2.034). Taking these two responses together, the results are: n, 10 A, 1.698 M, 1.568. The association seeking-hunt(ing) thus appears stronger than seeking-looking.

[3] 'suchen' does not occur as a stimulus word in the German investigation. Out of a total of 80 verbs which T&M used as stimulus words, I have taken only those which correspond to those used by me; whereas in the case of the other categories, where T&M use only 10 words, I have given them all.

Table V

Stimulus	Most Frequent Reaction				Next Most Frequent Reaction				All Remaining Reactions		
		n	A	M		n	A	M	n	A	M
1. *I[1] ....(46)	*you.........	27	1.358	1.265	me..........	6	1.666	1.653	13	1.678	1.381
2. *you..........(92)	me..........	26	1.357	1.274	they........	10	1.733	1.519	56	2.009	1.709
3. *we...........(91)	they........	28	1.404	1.403	us..........	20	1.540	1.414	43	1.794	1.450
4. *he...........(89)	*she.........	48	1.273	1.226	man.........	7	2.264	1.855	34	1.681	1.440
5. her..........(87)	she.........	27	1.357	1.267	him.........	23	1.765	1.558	37	1.935	1.771
6. it...........(90)	they........	20	2.240	1.856	that........	9	1.826	1.842	61	2.096	1.938
7. they.........(94)	them........	29	1.743	1.592	we..........	16	1.608	1.418	49	2.073	1.653
8. *this.........(95)	*that.........	68	1.165	1.097	what........	3	1.619	1.696	24	2.030	2.106
9. *that.........(97)	*this.........	47	1.307	1.211	{ which....... { what........	6 6	1.346 1.998	1.324 1.820	38	2.113	1.904
10. *what........(94)	when........	20	1.445	1.336	that........	14	1.953	1.584	60	2.043	1.724
1. ich...........(8)	du..........	4	1.25		er..........	3	1.53		1	3.00	
2. du..............	er..........	5	1.28		——	–	—		3	2.07	
3. wir..............	{ uns......... { ihr..........	3 3	1.47		——	–	—		2	2.30	
4. er...............	sie.........	4	1.30		——	–	—		4	2.75	
5. sie..............	er..........	3	0.93		——	–	—		5	1.76	
6. ihr..............	wir.........	3	1.60		{ sie......... { seid........	2 2	{ 1.60		1	5.00	
7. wer..............	er..........	3	1.60		ist.........	2	3.80		3	2.13	
8. dieser...........	jener.......	6	1.33		Mann.......	2	2.50		–	—	
9. jener............	dieser......	4	1.30		——	–	—		4	1.75	
10. was.............	das.........	4	1.60		——	–	—		4	1.80	

[1] The stimulus word 'I' was understood by many observers as 'eye.'

TABLE VI

Stimulus	Most Frequent Reaction				Next Most Frequent Reaction				All Remaining Reactions		
		n	A	M		n	A	M	n	A	M
1. *where........(98)	when.........	33	1.478	1.339	there..........	23	1.464	1.401	42	1.798	1.626
2. *here[1].........(64)	*there.........	37	1.420	1.159	now...........	10	1.391	1.222	17	1.820	1.935
3. *there[2]........(70)	*here..........	23	1.396	1.256	where.........	20	1.813	1.635	27	2.198	1.978
4. *now.........(94)	then.........	55	1.426	1.330	when..........	10	1.809	1.327	29	1.979	1.824
5. *then[3]........(68)	now..........	34	1.870	1.480	when..........	12	1.916	1.504	22	1.952	1.652
6. *when........(96)	now..........	21	1.737	1.559	where.........	20	1.453	1.333	55	1.776	1.485
7. *never........(89)	now..........	17	1.506	1.368	always........	15	1.649	1.444	57	2.249	1.733
8. *always.......(93)	*never.........	37	1.774	1.489	now...........	20	2.030	1.731	36	2.238	1.976
9. seldom.......(94)	often.........	42	1.815	1.548	never..........	14	1.818	1.768	38	2.261	1.950
10. often........(90)	seldom.......	20	1.573	1.496	soon..........	14	1.620	1.377	56	2.297	1.884
1. wo...........(8)	da...........	4	1.45		——	–	—		4	2.40	
2. hier..............	dort..........	6	1.37		——	–	—		2	2.30	
3. dort..............	hier..........	5	1.32		——	–	—		3	2.47	
4. jetzt.............	nie...........	2	7.30		——	–	—		6	1.87	
5. dann.............	wann.........	6	1.70		——	–	—		2	1.90	
6. wann.............	dann.........	5	1.68		wo............	2	1.50		1	8.00	
7. niemals..........	jemals........	5	1.72		——	–	—		3	2.87	
8. immer...........	nimmer.......	2	1.60		——	–	—		6	1.93	
9. ——[4]											
10. ——[4]											

[1] frequently understood as *hear*.

[2] frequently understood as *their*.

[3] frequently misunderstood as *them*.

[4] *selten* and *oft* do not occur as stimulus-words in the German investigation. T&M used 10 adverbs of place and 10 adverbs of time. Of these I have given above only those which correspond to my stimulus-words.

action-times, respectively, of the most frequent response, the next most frequent response and of all remaining responses. A star before a stimulus-word indicates that it occurs as a stimulus word in the experiments of Thumb and Marbe; a star before a most frequent response indicates that it was also the favored response in the German investigation. Below each table is given the corresponding German table.

Of the 60 stimulus-words used in my investigation, 50 correspond to stimulus-words used by Thumb and Marbe. Of the most frequent responses to these 50 words, 35, or 70%, agree with the favored German reactions. It should be noted, moreover, that in some cases where the favored English response is not the same as the favored German response, the next most frequent response of the one investigation agrees with the most frequent of the other. Thus the responses to *son* (*Sohn*) are *daughter* (*Vater*); *Tochter* however occurs as the next most frequent German response and *father* as the next most frequent English response. It is quite possible that had Thumb and Marbe used a larger group of observers, the agreement between the German and English results would have been still greater.

In both the English and German experiments, stimulus-words of a given category were responded to predominantly with words of the same category, as the following figures show. (German percentages are given in parentheses.)

1. Names of personal relations responded to names of personal relations: 77.4% (80%).

2. *Adjective responses to adjectives:* 85.3% (87.5%).

3. *Numeral responses to numerals:* 83.9% (87.5%).

4. *Pronoun responses to pronouns:* 72.9% (71.3%).

5. *Adverbs of place responded to adverbs of place:* 51.7% (68.8%).

Adverbs of time responded to adverbs of place: 26.3% (1.2%).

6. *Adverbs of time responded to adverbs of time:* 72.4% (76.3%).

Adverbs of place responded to adverbs of time: 6.9% (6.2%).

7. *Verb responses to verbs:* 70.2% (42.0%).
Noun responses to verbs: 23.3% (51.7%).

The English and German results are seen to agree closely in this respect except in the case of the responses to adverbs of place and to verbs. The English observers show a marked tendency to respond to adverbs of place with adverbs of time, whereas in the case of the German observers this tendency is hardly present. A decided difference is seen between the English and German responses to verbs. The results for the English observers are here similar to the results in the other categories, whereas only 42.0% of the German responses to verbs are again verbs. To what this difference is due may be inferred from a comparison of the results of Schmidt.[1] In this investigation, 8 boys, aged about 10 years, acted as observers. The stimulus-words were the forms of the present and imperfect indicative, the present infinitive and the past participle of 30 verbs. Of the responses to these words, 89.65% were verbs, 4.82% substantives and 5.53% scattering. Although Schmidt does not himself give us the necessary data, we may infer from a comparison of the results of Thumb and Marbe that most of the substantive and scattering responses were given as reactions to the 30 infinitives. "Das Resultat ist sprachpsychologisch nicht uninteressant; wir betrachten den Infinitiv als Träger der Verbalbedeutung, als die abstrakte Verbalform, und können daher a priori verstehen dass der Infinitiv mit sonstigen Wortklassen durch die Wortbedeutung assoziativ enger verknüpft ist als eine finitive Verbalform, deren assoziative Beziehungen mehr durch die formale Seite bestimmt sind."[2] Since the English observers were given participles as stimulus-words and the German observers infinitives, the difference in the results is probably due to the difference in the stimulus-words. If this is so, we see between the infinitive and finite forms, besides the linguistic difference, also a decided psychological difference.

It is to be noted also that of the responses to adjectives,

[1] *Ztschr. f. Pscyhol. u. Physiol. d. Sinnesorgane*, **28**, p. 65 ff.

[2] Thumb, *Indoger. Forsch.*, **22**, 35.

76.5% were adjectives of meanings *opposed* to those of the stimulus-words. For the German responses the percentage was 82.5%. In the case of the numerals, the most frequent response to each stimulus-word was the *next higher numerals* in the German results the response *zwanzig* to the stimulus *zehn* offers an apparent exception to this rule;[1] the next most frequent response is however *elf*.

There are 17 cases of reciprocal associations, as follows: father ⇄ mother, son ⇄ daughter, brother ⇄ sister, uncle ⇄ aunt, big ⇄ little, light ⇄ heavy, old ⇄ young, thick ⇄ thin, white ⇄ black, giving ⇄ taking, eating ⇄ drinking, losing ⇄ finding, reading ⇄ writing, this ⇄ that, here ⇄ there, now ⇄ then, seldom ⇄ often.

It remains for us to consider the relations between the reaction-times for the most frequent, next most frequent and remaining responses. Thumb and Marbe found in their experiments that the more frequent a response was, the shorter was its reaction time. The following table will show that the same rule applies to the English results. In the lines marked *a*, *b* and *c* are given the average reaction-times of the most frequent, next most frequent and remaining responses, respectively, to the stimulus-words of each of the six categories. At the right are given the medians of all six categories.

	Family Names	Adjectives	Numerals	Verbs	Pronouns	Adverbs	Total
(*a*)	1.387	1.258	1.211	1.577	1.465	1.600	1.416
(*b*)	1.822	1.477	1.767	1.720	1.756	1.696	1.706
(*c*)	2.180	2.254	1.982	2.052	1.945	2.057	2.078

Results of Groups B and C

In the following tables are given the results for Group B (children) and Group C (uneducated adults). The figures in parentheses after the stimulus-words indicate the total number of successful reactions; the columns under *n* and *M* give the number of occurrences and median time of the most

[1] I say 'apparent' because in the very common 'counting by tens' *twenty* may be regarded as the next higher numeral after *ten*. The English table also shows the influence of counting by twos and by fives.

Group B

	Stimulus	Most Frequent Reaction			Next Most Frequent Reaction			All Remaining Reactions	
			n	M		n	M	n	M
Table Ib...	1. father....(10)	*mother....	10	1.439	—	–	—	–	—
	2. mother...(11)	*father.....	10	1.456	son....	1	1.924	–	—
	3. son.......(6)	*daughter...	4	1.417	—	–	—	2	1.834
	4. daughter..(11)	*son........	9	1.921	—	–	—	2	1.556
	5. brother...(11)	*sister......	11	1.330	—	–	—	–	—
	6. sister.....(11)	*brother....	9	1.412	—	–	—	2	1.310
	7. cousin....(11)	*uncle......	3	1.485	niece...	2	1.928	1	1.590
		sister......	3	2.087	aunt...	2	4.734	–	—
	8. aunt.....(11)	*uncle......	9	1.707	—	–	—	2	3.338
	9. uncle.....(10)	*aunt.......	5	1.553	cousin..	3	1.559	2	2.120
	10. nephew...(11)	*niece......	7	1.593	cousin..	2	2.082	2	2.522
Table IIb..	1. big.......(11)	*little......	10	1.422	—	–	—	1	1.286
	2. little.....(11)	*big........	10	1.460	—	–	—	1	2.333
	3. light[1]								
	4. heavy....(11)	*light.......	8	1.654	—	–	—	3	1.774
	5. old.......(11)	*young.....	11	1.447	—	–	—	–	—
	6. young....(11)	*old........	11	1.398	—	–	—	–	—
	7. thick.....(10)	*thin.......	6	1.924	—	–	—	4	2.291
	8. thin.......(9)	*thick......	4	1.550	—	–	—	1	1.653
		fat........	4	2.518					
	9. white....(11)	*black......	9	1.164	blue....	2	2.012	–	—
	10. black.....(11)	*white......	9	1.296	—	–	—	2	1.714
Table IIIb..	1. one.......(9)	*two.......	8	1.437	—	–	—	1	1.586
	2. two......(11)	*three......	4	1.285	—	–	—	3	1.645
		four.......	4	1.986					
	3. three.....(11)	*four.......	8	1.397	—	–	—	3	1.893
	4. four.......(9)	*five.......	3	1.298	three...	2	1.890	4	3.542
	5. five.......(7)	*six........	4	1.433	—	–	—	3	1.333
	6. six.......(11)	*seven......	7	1.266	twelve..	2	3.586	2	1.417
	7. seven....(11)	*eight......	4	1.261	six.....	3	1.469	4	1.716
	8. eight......(8)	*nine.......	3	1.403	ten.....	2	2.745	3	2.286
	9. nine......(10)	*ten........	8	1.604	—	–	—	2	1.426
	10. ten.......(10)	*eleven.....	4	1.549	—	–	—	6	1.790
Table IVb..	1. giving....(10)	gave......	2	1.314	—	–	—	6	2.375
		keeping....	2	2.145					
	2. taking.....(9)	keeping....	2	3.308	—	–	—	7	2.321
	3. eating....(10)	*drinking...	3	1.971	ate.....	2	1.424	5	2.097
	4. drinking..(11)	*eating.....	3	2.221	drank..	2	1.644	4	4.072
					cup....	2	1.914		
	5. losing....(11)	*finding....	4	2.654	found...	3	2.227	4	1.522
	6. walking....(9)	*running....	3	2.241	slow....	2	2.838	4	1.789
	7. reading...(10)	*writing....	5	1.427	read....	2	1.204	3	3.053
	8. writing...(11)	*reading....	3	1.749	wrote...	2	1.428	4	3.366
					playing.	2	6.942		
	9. seeking....(9)	—	—	—	—	–	—	9	2.196
	10. finding...(10)	*losing.....	4	2.284	keeping	2	5.590	4	1.742

[1] Understood only once as 'not heavy'; response *heavy*, time 1.679. Understood 10 times as 'not dark'; response *dark* occurs 8 times, median time 1.365; time of other two responses 1.588.

Group B—*Continued*

	Stimulus	Most Frequent Reaction	n	M	Next Most Frequent Reaction	n	M	All Remaining Reactions n	M
Table Vb...	1. I.........(6)	*you.......	3	1.532	——	–	——	3	2.249
	2. you......(10)	*me........	4	1.350	——	–	——	6	2.418
	3. we........(9)	*they.......	2	1.469	——	–	——	5	2.834
		you.......	2	2.169		–			
	4. he.......(10)	*she........	7	1.555	her.....	2	1.449	1	.953
	5. her......(11)	him.......	4	1.841	he.....	2	2.046	3	2.097
					his.....	2	1.547		
	6. it.........(8)	is.........	2	3.298	——	–	——	6	1.876
	7. they.....(10)	*them......	4	2.179	——	–	——	6	1.942
	8. this......(11)	*that.......	7	1.267	——	–	——	4	2.300
	9. that......(11)	*this.......	4	2.002	there...	2	2.238	5	1.894
	10. what.....(9)	who.......	2	1.514	——	–	——	5	2.446
		that.......	2	2.097					
Table VIb..	1. where....(11)	there......	6	1.538	when ..	2	1.876	3	3.237
	2. here.......(8)	*there......	5	1.851	where..	2	1.590	1	2.088
	3. there.....(10)	*here.......	4	1.618	then....	2	1.550	4	3.758
	4. now......(11)	*then.......	7	1.607	——	–	——	4	2.212
	5. then......(8)	*now.......	4	1.646	——	–	——	4	1.752
	6. when.....(10)	*now.......	4	1.935	——	–	——	6	1.836
	7. never....(10)	*now.......	2	2.047	——	–	——	6	2.388
		always.....	2	2.928					
	8. always....(10)	*never......	4	2.024	now....	2	1.568	4	2.322
	9. seldom....(7)	*often......	5	2.267	——	–	——	2	4.738
	10. often.....(10)	slow.......	2	2.814	——	–	——	6	7.378
		soon.......	2	3.464					

Group C

	Stimulus	Most Frequent Reaction	n	M	Next Most Frequent Reaction	n	M	All Remaining Reactions n	M
Table Ic....	1. father....(15)	*mother....	10	1.463	——	–	——	5	4.525
	2. mother...(15)	*father.....	8	1.916	sister...	3	1.967	4	2.040
	3. son.......(5)	*daughter...	3	2.873	——	–	——	2	2.232
	4. daughter..(15)	brother....	5	2.749	father..	3	1.726	7	1.814
	5. brother...(15)	*sister......	8	1.424	——	–	——	7	2.390
	6. sister.....(15)	*brother....	11	1.669	——	–	——	4	1.982
	7. cousin....(15)	*uncle......	5	2.848	aunt...	4	1.653	6	2.516
	8. aunt.....(13)	*uncle......	6	1.482	no.....	2	1.758	5	1.748
	9. uncle.....(14)	*aunt......	4	2.132	sister...	2	1.298	4	2.278
		cousin.....	4	2.624	——				
	10. nephew...(14)	*niece......	4	1.952	——	–	——	6	1.923
		uncle......	4	2.156					
Table IIc ..	1. big......(12)	*little......	5	1.847	small ..	3	1.467	4	1.814
	2. little.....(14)	large......	6	2.285	big.....	3	1.651	5	1.501
	3. light[1]								
	4. heavy....(15)	*light......	8	1.675	load....	2	1.942	5	2.063
	5. old.......(14)	*young.....	10	1.386	——	–	——	4	2.277
	6. young....(14)	*old........	12	1.492	——	–	——	2	1.279
	7. thick.....(14)	*thin.......	10	2.170	——	–	——	4	1.496
	8. thin......(12)	*thick......	7	1.405	——	–	——	5	3.415
	9. white....(15)	*black......	8	1.780	red.....	2	1.775	3	2.504
					blue....	2	2.966		
	10. black.....(14)	*white......	7	1.534	yellow..	2	1.987	5	1.721

[1] Understood 3 times as 'not heavy'; responses and reaction-times as follows: *heavy*, 2.719; food, 2.524; clothes, 3.704. Understood 4 times as 'not dark'; response *dark*, median time 1.468. Understood as noun 8 times; response *darkness* occurs twice, time 1.808; median time of other responses 1.598.

GROUP C—*Continued*

	Stimulus	Most Frequent Reaction			Next Most Frequent Reaction			All Remaining Reactions	
			n	M		n	M	n	M
Table IIIc..	1. one......(15)	*two.......	8	1.506	four....	2	1.842	5	1.818
	2. two......(15)	*three......	5	1.116	——	–	——	5	2.115
		four.......	5	1.828					
	3. three.....(15)	*four.......	7	1.374	five....	2	2.134	4	3.104
					six.....	2	1.767		
	4. four.......(9)	six........	4	2.256	——	–	——	5	2.464
	5. five......(12)	*six........	5	1.932	eight...	2	2.790	5	1.957
	6. six.......(15)	*seven......	4	1.352	ten.....	3	2.175	8	1.985
	7. seven....(15)	*eight......	10	1.688	——	–	——	5	1.398
	8. eight.....(13)	*nine.......	3	1.536	ten.....	2	1.268	8	2.410
	9. nine......(15)	*ten........	5	1.459	——	–	——	10	1.994
	10. ten......(15)	*eleven.....	4	1.427	nine....	2	1.226	7	2.553
					twelve..	2	2.955		
Table IVc..	1. giving....(15)	*taking.....	6	1.394	——	–	——	9	1.990
	2. taking....(14)	something..	3	1.934	giving..	2	2.362	9	1.563
	3. eating....(15)	*drinking...	8	2.065	——	–	——	7	1.863
	4. drinking..(14)	*eating.....	2	2.164	——	–	——	10	2.160
		whiskey...	2	2.591					
	5. losing....(15)	*finding....	4	1.652	found...	3	2.040	5	1.634
					lost....	3	1.876		
	6. walking...(14)	*running....	5	1.421	talking.	3	1.457	6	1.845
	7. reading...(15)	*writing....	5	1.432	——	–	——	5	2.222
		spelling....	5	2.400					
	8. writing...(14)	*reading....	5	1.627	spelling.	3	1.800	6	2.639
	9. seeking...(15)	*finding....	3	1.770	——	–	——	9	1.729
		looking....	3	3.046					
	10. finding...(15)	lost.......	4	1.478	losing...	3	1.840	8	1.672
Table Vc...	1. I.........(8)	*you.......	2	2.265	——	–	——	6	1.567
	2. you......(14)	*me........	4	1.692	——	–	——	10	2.340
	3. we.......(14)	those......	2	1.438	——	–	——	10	1.589
		she........	2	1.567					
	4. he.......(13)	*she........	6	1.238	——	–	——	7	1.689
	5. her......(10)	*she........	4	1.372	him....	2	2.890	4	2.934
	6. it........(15)	that.......	2	1.662	——	–	——	11	2.421
		now.......	2	2.821					
	7. they.....(13)	*them......	3	2.180	——	–	——	10	1.553
	8. this......(14)	*that.......	8	1.750	——	–	——	6	2.457
	9. that......(15)	*this.......	4	1.450	they....	2	1.196	9	2.296
	10. what.....(15)	now.......	2	2.325	——	–	——	9	1.555
		who.......	2	4.922					
		is.........	2	2.044					
Table VIc..	1. where....(15)	there......	4	1.494	here....	3	3.264	8	1.762
	2. here.......(7)	*there......	3	1.656	——	–	——	4	1.796
	3. there.....(12)	*here.......	3	2.342	where..	2	2.664	7	2.075
	4. now......(13)	*then.......	8	1.701	——	–	——	5	2.102
	5. then.....(11)	*now.......	3	1.745	——	–	——	8	2.243
	6. when.....(13)	*now.......	5	2.071	where..	2	3.847	4	1.512
					anytime	2	3.041		
	7. never....(15)	*now.......	3	1.335	——	–	——	12	2.046
	8. always...(15)	forever....	2	2.376	——	–	——	11	2.033
		now.......	2	3.831					
	9. seldom...(12)	*often......	3	2.280	never...	2	1.739	7	2.661
	10. often.....(14)	*seldom....	3	1.702	now....	2	2.278	7	3.912
					quick...	2	3.876		

frequent, next most frequent and remaining responses. A star before a most frequent response indicates that it was also the favored response for Group A.

Except in the length of the reaction-times, the results of Groups B and C present comparatively small differences from those of Group A. Of the most frequent responses of Group B, 86.4% are the same as the most frequent responses of Group A. Of the most frequent responses of Group C, 83.1% are the same as those of Group A. The responses of Groups B and C, as was the case in Group A, are predominantly of the same category as their stimulus-words. In the category of numerals, the influence of counting by twos is to be noted; in Group B *four* occurs 4 times as a response to *two*, while in Group C *four* occurs 5 times as a response to *two*, and *six* occurs 4 times as a response to *four*. In both of these groups, *there* occurs as the most frequent response to *where;* the most frequent response of Group A was *when*. The supplementary groups are not large enough, however, to permit us to make comparisons between the results for single words. They do however show us clearly that the associative processes of children and uneducated adults do not differ widely from those of educated adults, except that they are slower. The following tables give the medians of the reaction-times, (a) of the most frequent, (b) next most frequent, and (c) remaining responses to the stimulus words of each of the six categories; at the right of each table are given the medians of the most frequent, next most frequent and remaining reactions of all the categories taken together.

Group B

	Family Names	Adjectives	Numerals	Verbs	Pronouns	Adverbs	Median
(*a*)	1.485	1.454	1.403	2.183	1.698	1.980	1.598
(*b*)	1.924	2.012	2.318	1.914	1.796	1.579	1.914
(*c*)	1.834	1.744	1.681	2.258	2.173	2.355	2.097

Group C

	Family Names	Adjectives	Numerals	Verbs	Pronouns	Adverbs	Median
(*a*)	2.042	1.675	1.506	1.770	1.721	1.745	1.702
(*b*)	1.726	1.858	1.988	1.858	2.048	3.041	1.954
(*c*)	2.136	1.814	2.054	1.854	1.992	2.061	2.033

It will be seen from these figures that the reaction-times for children and uneducated adults are longer than those for educated adults. That the times for the next most frequent responses are in many cases longer than those for the infrequent (remaining) responses and in a few cases shorter than the corresponding times for the most frequent responses, is due to the very small number of next most frequent responses (cf. the tables). The number of these being small, the medians represent, not the frequent values of a large group of observers, but the individual peculiarities of two or three observers. In Group A, where many observers were used, the relations of time between most frequent, next most frequent and infrequent reactions are clearly seen.

In discussing the associations of children, Thumb[1] calls attention to the investigation of Ziehen[2] and those of Watt.[3] Of the former investigation, Thumb says, "Wie schon Ziehen an Kindern zwischen 8 und 14 Jahren beobachtet hat, sind Verbal-, d. h. reine Wortassoziationen überhaupt selten; am haüfigsten sind Wörterergänzungen (Post-karte); gelaüfige Wortverbindungen und Reimassoziationen sind sehr viel seltener als bei Erwachsenen; wir sehen also schon hieraus, dass bei Kindern die Bedingungen viel seltener erfüllt sind, die wir für das Zustandekommen von Analogiebildungen voraussetzen müssen: Gelaüfigkeit, Schnelligkeit und Spontaneität der Assoziationen." The investigation of Watt had for its object a direct comparison of the results from children and adults. The stimulus-words used were the same as those which Thumb and Marbe had employed. Of the 8 observers, 5 were children in the second to fifth years of school. The results of this experiment show that the responses of the children are much more scattering than those of the adults; of the responses of the latter, 74% belonged to the class of 'most frequent' reactions; of the responses of the children, only 29% belonged to this class.

A study of Table B will show that the differences between

[1] *Indogerman. Forsch.*, 22, 43 f.

[2] *Ideenassoziation des Kindes, I, II*, Berlin, 1898, 1900.

[3] *Ztschr. f. Psychol.*, 36, 417 ff.

the associations of adults and children, found by Ziehen and Watt, do not appear in the results from the English observers —with one exception, namely, the length of the reaction-times. The tables given on pp. 484, 480, show that the association-times for children are longer than those for adults. But in the *character* of the associations, a comparison of the tables of Groups A and B shows a remarkable similarity; thus, 86.4% of the most frequent responses of Group B are the same as the most frequent responses of Group A. The greater *scattering* of the responses which Watt found in children our results do not show at all. Indeed, the scattering is less in the case of the children than in that of the adults. The following table will show what percent of all responses in Group A (educated adults), Group B (children) and Group C (uneducated adults) belong (*a*) to the class of 'most frequent' responses and (*b*) to the classes of 'most frequent' and 'next most frequent' taken together.

	A	*B*	*C*
(*a*)	51.7	58.0	42.8
(*b*)	63.8	67.6	52.9

These figures show that the scattering is greatest in the uneducated adults and least in the children. Of course, it must be borne in mind that Groups B and C are much smaller than Group A; nevertheless, until further and more extended investigations are made, we cannot regard the results of Ziehen and Watt as being of general significance. The significance of the conclusions reached from experiments of this sort is proportional to the number of observers. Until more reliable results are at hand, we are justified in believing that the associations of children are quite similar to those of adults, except in the length of the reaction-time. That there are *more* associations present in the adult than in the child we cannot doubt; but the associations which the child already has are similar to those of the adult.

General Conclusions

1. The rule established by Thumb and Marbe, namely, that the more frequent an association is, the more rapidly

does it take place, is confirmed by the results from the English-speaking observers.

2. In both languages, words of a given category are associated predominantly with words of the same category.

(*a*) In all categories investigated, with the exception of the numerals, reciprocal associations were found. In these cases a word *a* which calls up a word *b* is in turn called up by *b*.

(*b*) Numerals are associated predominantly with higher numerals; the numerals 1–10 are associated predominantly with the *next* higher numeral.

(*c*) Adjectives are associated predominantly with adjectives of opposed meaning.

3. A comparison of the English and German results tends to show that the associations of English- and German-speaking communities correspond in the case of most words which are of familiar meaning and in universal use in both languages.

4. The reaction-times of the associations of children and uneducated adults are longer than those of educated adults, but the favored associations are in most cases the same, and the essential character of the associations is similar.

JUDGMENTS OF FACIAL EXPRESSION AND SUGGESTION

BY HERBERT SIDNEY LANGFELD

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In an experiment upon the judgment of emotions from facial expressions,[1] in which the subjects attempted to name the emotion depicted in a number of photographs, it was noticed that it was at times possible under suggestion to read various emotions in the face. An experiment was therefore arranged to determine to what degree one is open to suggestion when reading character. It was also the aim of the experiment to bring out, if possible, individual differences in the power of interpretation of facial expression and in suggestibility. The same pictures were used as in the former experiment. Four of the pictures are reproduced in Plate I. These were modified photographs of a talented actor, which appeared in the book by Rudolf entitled 'Der Ausdruck des Menschen.' One hundred and five of the pictures were selected as being the best for the experiment. They covered a wide range of emotions and moods. A few of bodily pain and of the sensations of smelling and tasting, and so forth, were included. Miss Grace Speir conducted the experiment, and tabulated the results. There were five subjects, who were all either members of the advanced experimental course or graduate research students.

The experiment extended throughout the second semester of 1916–1917. Each subject came for one hour a week, and as many pictures as possible were presented in that hour. The subject was shown the picture, and asked to write down his judgment of the expression. After he had done this, he was told either the artist's title of the picture, or an incorrect title, such as 'inspiration' for a picture of 'distrust,' and asked whether he agreed with this title. Some of the incorrect

[1] A report will appear in the *J. of Abnorm. Psychol.*

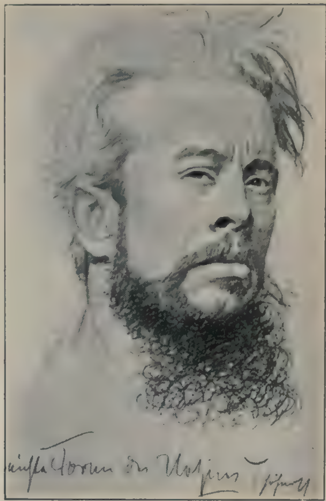
PLATE I

3

L, 3 F

Bedenklichkeit. Kritisch

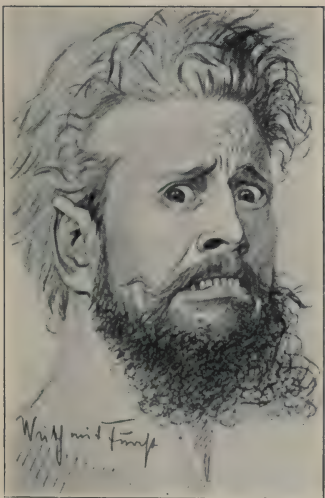
401



XXV, 1 A

Hohn. Leicht

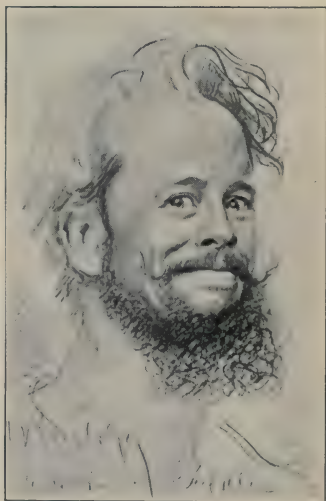
510



XXVI, 67 A

Wut. Mit Furcht
(leichter Ausdruck)

585



XXIX, 1 F

Lachen.
Bedeutungsvolles Lächeln

FIG. 1. Four of the pictures used. They represent: 3, doubtful, critical; 401, mild scorn; 510, rage with fear; 585, significant smile.

titles were as opposed as possible to the correct title; others were rather similar. The series of one hundred and five pictures was gone through twice. At one presentation the subject was told the correct title, at another presentation the incorrect. On some pictures the correct title was given at the first presentation,—on others at the second presentation, so that the subject never knew, even if he did suspect the purpose of the experiment, whether a right or wrong title was being suggested.

The results are contained in Table I. The various groups of emotions used are shown in the first column. In the second column is the number of tests for each group of emotions. As there are five subjects, these numbers must be divided by five to give the number of pictures in a group; for instance, the scorn-contempt group has 80 judgments and 16 pictures.

The third column shows the number of times the subjects approximated the actual title of the book. An approximated

TABLE I

1	2	3		4		5		6		7		8		9		10	
Expression	Number of Judgments	Book Title Approximated		Of Column 3 Book Title Later Approved		Of Column 4 Wrong Title Later Suggested and Approved		Book Title Not Approximated		Of Column 6 Book Title Later Approved		Of Column 7 Wrong Title Later Suggested and Approved		Total Book Title Approved		Total Wrong Title Suggested and Approved	
		%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.
Scorn-Contempt.	80	36	29	96	28	21	6	64	51	80	41	54	22	86	69	41	28
Misgivings......	65	32	21	100	21	24	5	68	44	63	28	21	6	75	49	23	11
Aversion-Hate...	55	38	21	100	21	42	9	62	34	70	24	62	15	82	45	53	24
Amazement.....	55	58	32	90	29	21	6	42	23	60	14	71	10	78	43	37	16
Laughter........	50	64	32	100	32	31	10	36	18	55	10	70	7	84	42	40	7
Distrust........	45	31	14	92	13	61	8	69	31	54	17	71	12	67	30	67	20
Anger-Rage.....	30	30	9	88	8	0	0	70	21	57	12	25	3	67	20	15	3
Anxiety—Fear-Terror......	25	36	9	88	8	37	3	64	16	68	11	45	5	76	19	42	8
Inspiration......	25	8	2	100	2	100	2	92	23	47	11	64	7	52	13	69	9
Covetousness....	25	4	1	100	1	0	0	96	24	45	11	36	4	48	12	33	4
Wicked-Ill-tempered....	20	15	3	100	3	33	1	85		64	11	45	5	70	14	43	6
Begging-Entreaty	10	10	1	100	1	100	1	90	9	77	7	57	4	80	8	63	5
Bodily Pain.....	10	50	5	100	5	0	0	50	5	40	2	0	0	70	7	0	0
Smelling-Tasting, etc.........	30	37	11	100	11	9	1	63	19	78	15	73	11	87	26	46	12
Average-Total.	525	32	190	97	183	34	52	68	335	61	214	49	111	73	397	41	153

title was taken as a correct judgment because it could not be expected that the subject would frequently use the exact words of the author in describing the picture, so that 'approximated' means 'equivalent.' Throughout this table, the data are presented both in actual amounts and percentages. In the 80 judgments of the scorn and contempt group, for example, 29, or 36%, were correctly approximated. It will be seen that the laughter group was the most readily interpreted, and that amazement comes next. Scorn and contempt, misgiving, aversion and hate, disgust, anger and rage, anxiety, fear, and terror, all of which are more or less related, are interpreted with about the same accuracy. Inspiration, covetousness, wicked and ill-tempered, begging and entreating were poorly interpreted. It was not expected that the sensation groups would give so low a percentage of correct judgments. In this group were also included pictures of sneezing and yawning, and even these were at times incorrectly named. Of the entire 525 judgments a little over a third were correct.

In the fourth column is the number of book titles approved of those which had been already approximated, that is, if we take the scorn-contempt group, the artist's title for 28, or 96%, of the 29 pictures whose titles had been approximated was approved when subsequently shown by the experimenter. These figures give us a check upon the cleverness of the artist in portraying the desired emotions, and it is the data in the fourth column that are used as a basis in determining the suggestibility. A comparison of the totals in columns three and four shows that of the total of 190 such approximations only seven of the artist's titles were not approved when subsequently shown.

The figures in the fifth column show the degree of suggestibility for the various emotions. In the first group, 28 titles were approved. When the wrong titles came to be suggested for these same 28 pictures (after an interval of a month, on an average) six of these wrong titles were accepted. Of the 183 pictures whose titles had been already approved the suggested wrong title was accepted 34% of the time. Excluding the inspiration and begging-entreating groups,

which had only one and two judgments respectively as a basis for calculating the suggestibility, the distrust group offered the greatest opportunity for the effect of suggestibility, and the aversion-hate group was next. With anger and rage, suggestion had no effect.

The sixth column shows the number of times the subjects were unsuccessful at approximating the picture. These numbers are complements of the figures in column three.

Column seven gives the number of artist's titles which were approved of those pictures which the subjects themselves had previously not approximated. For instance, in the first group, of the 51 pictures which had not been approximated, the subjects approved 41 of the book titles. As was to be expected, fewer of the artist's titles were approved of these non-approximated pictures than of the approximated ones; in all only 61%, as against 97% of the approximated titles. This drop is indicative of the artist's failure to reproduce the expression he desired. The pictures that were not approximated, even though the artist's titles were approved, gave more room for suggestion than the approximated pictures. 49% of the suggested wrong titles of the 214 approved, non-approximated titles were accepted as against 34% of the 183 approved and approximated. This means that if the subject had not himself judged the title correctly, even though he agreed with the title that the artist gave, he was more open to subsequent suggestion than when he had judged the title correctly in the first place. Distrust is again high in suggestibility. The suggestibility in amazement is as high as in distrust, and in laughter almost as high.

In the next to the last column is the sum of all the book titles which are approved whether they had previously been approximated or not, and is obtained by adding columns seven and four.

In the last column is the total amount of suggestibility with the pictures from which the figures of the previous column were obtained, that is, the total amount of suggestibility whether the titles had previously been approximated or not. Of the total number of 397 titles approved, 153 or 41% offered opportunity for suggestion.

Table II shows individual differences both in the ability to judge emotions and in suggestibility. In the first horizontal line are the five subjects A, B, C, D, and E.

TABLE II

Subject	A		B		C		D		E	
	%	No.	%	No.	%	No.	%	No.	%	No.
Book title approximated	55	58	30	31	39	41	29	31	17	18
Of last, book title later approved	97	56	90	28	88	36	100	31	94	17
Of last, wrong title later suggested and approved	29	16	32	9	31	11	16	5	47	8
Book title not approximated	45	47	70	74	61	64	70	74	83	87
Of last, book title later approved	68	32	58	43	53	36	47	35	92	80
Of last, wrong title later suggested and approved	34	11	46	20	44	16	31	11	69	55
Total: book title approved	84	88	68	71	69	72	63	66	92	97
Total: wrong title suggested and approved	31	27	41	29	37	27	24	16	65	63

In the second horizontal line are the number and percentage of the book titles approximated by each subject. Subject A is the best and subject E the worst in correctly interpreting facial expressions. In order of merit they rank A, C, B, D, E.

The third horizontal line shows the number of book titles which had been previously approximated, and which were approved by the various subjects. For example, A approximated 58, or 55% of the total number. This column offers no new facts, but is used as a basis for calculating the amount of suggestibility which is shown in the next column.

Here it will be seen that subject E is the most suggestible having accepted 47% of the titles suggested with pictures whose correct titles he had approved; that is, when he was given incorrect titles in connection with the 17 pictures whose correct titles he had previously approved, he accepted eight of them. Subject D was the least suggestible. The ranking of the subjects in suggestibility is E, B, C, A and D.

In the fifth horizontal line is the number of book titles which the subjects did not approximate. This is the complement of the results in the second horizontal line. Therefore the ranking by subjects is the reverse of that of the second horizontal line.

In the sixth horizontal line is the number of book titles which were approved of those which had previously not been approximated. It is seen that subject E is the least discriminating, accepting almost every title shown him, that is, 80 or 90% of the titles suggested for the 87 pictures, previously not approximated. In this group of pictures whose titles were not approximated there are, of course, a great many pictures which very poorly portray the emotion intended. Undoubtedly, when the book titles were subsequently given, some of these which are really inappropriate were accepted through suggestion. If we rank the subjects according to the number of those book titles which they accepted, we shall see that the order closely correlates with that of the ranking according to suggestibility; the most highly suggestible accepting the most titles, and the least suggestible accepting the fewest. Inasmuch as subject E is also the most suggestible to wrong titles, it may be said that he accepts almost anything given him whether right or wrong.

The seventh horizontal line shows the amount of suggestibility to these non-approximated titles according to subjects. It will be remembered from the first table where the results of the five subjects were averaged, that there was more suggestibility in the cases where the book title was not approximated. The figures of this line show that this is so in the case of each individual subject. The ranking according to suggestibility is the same as that previously shown with the approximated titles, being subjects E, B, C, A, D.

The last two horizontal lines present the average of the results of the approximated and non-approximated groups and offer no new facts.

It may be said that by this experiment decided individual differences have been shown among the five subjects in regard to suggestibility and ability to read facial expression. It is evident that subject E has very little ability in reading faces, and is highly suggestible, accepting 65% of the wrong titles shown him, as well as 92% of the book titles, some of which were decidedly poor according to the other subjects. It was discovered from other unrelated experiments and from the

subject's own report that he had little if any visual imagery, and that he felt entirely at sea when asked to make a judgment of emotions visually expressed. It does not follow, however, because one is unable to make correct judgments that one is highly suggestible, and will accept any title offered. Subject D, for instance, ranks next to E in inability to interpret correctly the emotions, but he accepts the fewest wrong titles. He also rejects the greatest number of titles in the non-approximated group. There is a suspicion that he is of the negatively suggestible type. The fact that he accepted all the book titles in the approximated group when they were read to him, subsequent to his judgment, does not contradict this, as he was here accepting titles similar to those that he himself had already made. Nor does it follow that the subject who gives the greatest number of correct judgments is the least suggestible. Subject A is more suggestible than subject D, although he has the greatest number of correct judgments. The other subjects, B and C, hold a middle place in regard to both correct judgments and suggestibility.

The results of this experiment seemed sufficiently encouraging to warrant the devising of a test along these lines. With this purpose fourteen of the best pictures have been selected.[1] The titles of these pictures have been approved by at least four of the five subjects. It is now the intention to test with them a larger number of subjects.

[1] These pictures can be obtained from the author, at cost, by anyone who desires to use them.